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PROCEEDINGS

FOURTH MEETING, SECOND HALF, 64TH SESSION

In the Chair:

THE PRESIDENT, COLONEL THE MASTER OF SEMPILL

A meeting of the Royal Aeronautical Society was held in the Lecture Theatre of the Royal Society of Arts, 18, John Street, Adelphi, London, W.C.2, on Thursday, March 7th, 1929.

The PRESIDENT: Unfortunately the author had sustained an accident, upon which influenza had developed, and could not be present to read his paper in person. But Mr. Bateman, who had been associated with him in this particular work, had kindly consented to read the paper in Mr. Lock's unavoidable absence.

AIRSCREW BODY INTERFERENCE

BY

C. N. H. LOCK, M.A.

Foreword

The present paper deals with a semi-empirical method of using airscrew strip theory to take account of the mutual interference of a body and tractor airscrew. It also contains a brief account of the successive wind tunnel experiments starting from the original tests of the "family of airscrews," which have led up to the theory and which provide the evidence for its accuracy over the somewhat limited range which they cover. The majority of the work has been published at intervals extending over the past six years and I have to thank the Aeronautical Research Committee for permission to include some recent experimental work not yet published.

The earlier experiments were carried out in collaboration with Mr. Fage, A.R.C.Sc., who also gave valuable advice in the later stages. Mr. Bateman, B.Sc., collaborated in the whole of the work up to the time of his temporary absence from the laboratory in 1926-28. He was jointly responsible for the earlier form of the theory of body interference here described, and carried out in collaboration with Mr. Townend the experiments on which the final form of the theory was based. The recent experiment of pressure plotting a body with tractor airscrew was carried out in collaboration with Mr. Johansen, B.Sc. Finally I have to thank the late and present Superintendents of my Department, Mr. Southwell and Mr. Relf, for their valuable advice and suggestions during the progress of the work.

1. Introduction

The problem of airscrew body interference is of importance in various ways. Firstly, it is necessary for the designer to know how the performance of a given airscrew and body will be altered when they are associated in a given relative position; in particular, how the presence of the body will affect the torque of the airscrew or the power required for a given speed of rotation, the determination of the airscrew thrust being of less practical importance. The second and more important aspect of the problem is to determine the design and relative position which will give the best performance. Is it better aerodynamically and apart from structural considerations to place the screw clear in front of the body, or to house it near the nose, continuing the contour of the body through it by means of a spinner, or further back still near the maximum ordinate, or finally is the pusher type the most efficient? We require also the effect of varying the relative size of body and screw to indicate the advantages to be obtained from the use of reduction gearing.

A further problem is to decide the effect of the screw on the streamline characteristics of the body; whether a streamline shape will retain its low drag coefficient when a rotating screw is placed in front of it, or is housed in the nose by means of a spinner; whether the shape which gives the lowest drag when a screw is running differs from the shape which has proved to be the best in the absence of a screw; or whether the good streamline flow is bound to be upset by a screw so that it is not worth while to take extreme precaution in streamlining those parts of the structure which lie in the slipstream. In practice the ideal streamline form of the body is necessarily spoilt by the cockpit, cylinder heads or radiators, wing roots and other excrescences, and it is required to deduce their effect on performance from observations in the absence of the airscrew.

To obtain definite quantitative results as to body interference it is almost essential to make use of systematic model experiments in a wind tunnel. It is possibly even more difficult than in most aerodynamic problems to obtain definite conclusions from full scale experiments both on account of the diversity and complication of the shapes of actual aeroplane bodies, and also the comparative smallness of the effects to be observed. It has been usual to exaggerate somewhat the size of the body in relation to the screw as compared with practice, in order to magnify the differences to be observed; the conclusions may be expected to hold good for the relatively smaller bodies of actual machines, and may be applied to them with confidence by means of the theory about to be described. The ratio of maximum body to airscrew diameter is 0.6 and 0.64 in the majority of the experiments here described, while the corresponding value for the machines illustrated in Fig. 26 varies from .46 to .5.

The model experiments on airscrew body interference have been made on a few simplified and idealised shapes, on the principle of varying one condition at a time, such as the relative size of airscrew and body, or the position of the airscrew in the fuselage.

2. Preliminary Discussion of Interference

It is convenient to define and subdivide the mutual interference of a tractor airscrew and body as follows. As a standard of reference we may consider the airscrew and body moving at normal speed, but completely separated from each other. Next we may imagine the airscrew situated at some distance in front of the body so that the latter obtains the full benefit of the draught, but is too far away to affect appreciably the performance of the screw. The drag of the body will be increased owing to the increased velocity of the slipstream and also possibly on account of its increased turbulence or rotation. We may speak of this effect on the body drag as the slipstream effect, and if necessary distinguish the two parts as velocity effect and turbulence or spoiling effect. If now the screw is brought nearer to the body, there appears an additional reaction between

the two. The effect of the body on the screw is represented mainly as a change in the relative velocity through the airscrew disc; if the screw is near the nose, the velocity is in general reduced, the effective angle of incidence of each blade element is increased, and the thrust and torque of the screw are increased. The corresponding effect of the screw on the body is due to the increased static pressure immediately behind the screw; this increased pressure acting on the nose of the body will increase its resistance above the value due to the slipstream. It is an experimental fact that the resultant pressure over the nose of a streamline body is generally very small or slightly negative, and this should also be true when the body is in the slipstream at a distance behind the screw. This fact may be useful in interpreting experimental results since the pressure drag on the body corresponding to the increased thrust on the screw may be taken as equal to the whole of the resultant pressure on the forward half of the body. Although the increased thrust and the "pressure drag" are apparently produced by diverse causes, it is to be expected on the principle of the equality of action and reaction that they will to some extent balance one another. How and to what extent they do so will be explained later.

A point may be mentioned here which seriously increases the difficulty of producing a satisfactory quantitative theory. The reduction of velocity due to the body is greatest near the surface, and falls off rapidly with increasing radial distance. It follows that a screw of small diameter will have a much greater proportion of its area in the region of reduced velocity than a larger screw; if a theory of interference is to account satisfactorily for the performance of screws

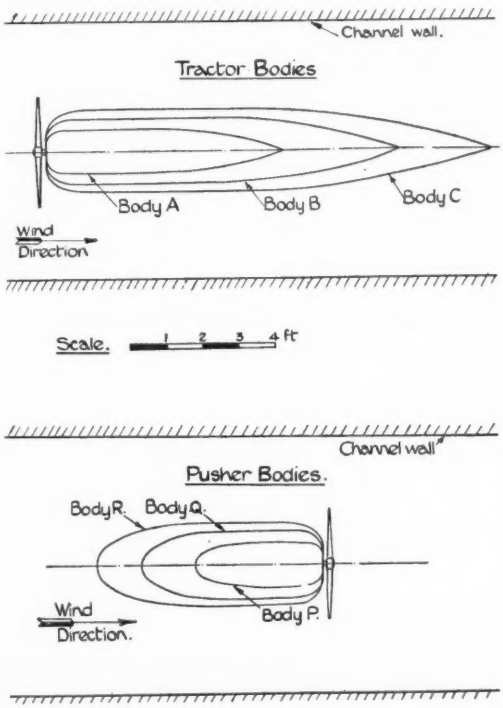

FIG. 1.

of varying relative diameter it is essential to treat the blade elements at different radii separately. It has been found possible to do this, but the necessity for doing so greatly increases the labour of computing the performance in any practical case.

3. *Wind Tunnel Experiments*

It is convenient at this point to give a short summary of the successive wind tunnel experiments which have led up to a consistent theory of airscrew body interference. Typical experiments will be described and their results illustrated by diagrams.

Two extensive programmes of research on performance of model bodies and airscrews have been carried out which were separated by a considerable interval of time. The first programme referred to the tractor shapes shown in Fig. 1 and Fig. 16C with external screw and included variations of pitch and number of blades of the airscrew and of the relative size of airscrew and body. The

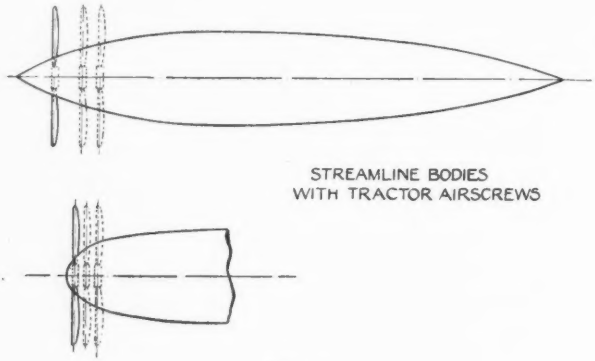

FIG. 2.

shape was a solid of revolution having a very bluff nose, and was designed to imitate the Bristol Fighter body F2B with nose radiator; it will be referred to as the bluff-nosed body. The same programme included experiments on the pusher shape, also shown in Fig. 1; this was found to give a considerably lower overall efficiency than the corresponding tractor combination, but this is sufficiently explained by the very bluff rear surface just in front of the airscrew and the result has no bearing on the question of the efficiency of a reasonably streamlined pusher body. These tests included the measurement of overall thrust, torque and body drag for given forward velocity and rotational speed. The second programme of experiments involved the shapes shown in Fig. 2 and Figs. 19A and 19B; the tractor airscrew was in all cases housed within the nose of the body by means of a spinner. Two reasonably well streamlined shapes were chosen having blunt and pointed noses; the main object of the programme was to discover the effect of altering the axial distance of the airscrew from the nose as shown in the figure. It was impossible to separate directly the body drag from the airscrew thrust, and in the initial experiments the resultant force (nett thrust) on the combination was alone measured.

Fig. 1 suggests that there must be an appreciable turbulence in the narrow space where the air is " trapped " between the airscrew and the front of the body, and that this will cause a loss of energy which may be augmented by the drag of the exposed boss and by the stalling of the blade roots. At the time when the earliest form of the present theory was developed, the only experiments

available related to this type of blunt nose and external airscrew, and it is perhaps unfortunate that all the observations of air flow about to be described were also confined to the same type. It is not surprising that better agreement with theory was obtained with the type shown in Fig. 2, where the screw is housed in the nose by means of a spinner. This arrangement has the double advantage that the boss and inefficient blade roots are eliminated, and the shape of the nose approximates more closely to the general notion of a streamline form. The latest practice in design appears to acknowledge this advantage by favouring the use of a spinner, and it may be taken that this type was in mind in developing the latest form of interference theory.

The next type of model experiment to be undertaken involved the measurement of the variation of thrust along a blade (thrust grading); the total thrust grading at a given radius is equal to the mean discontinuous change of pressure across the airscrew disc. The experimental method of measuring this is well known, and depends on the principle that the pressure in an open-ended tube

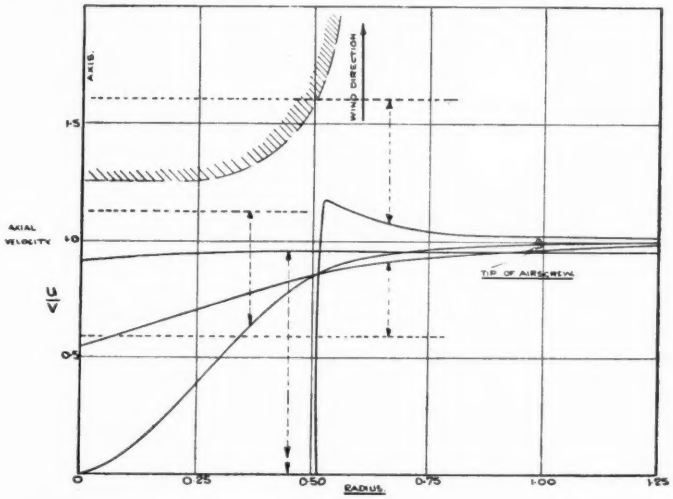

FIG. 3.

Observed axial component velocity in various sections normal to the axis. Body B without airscrew.

pointed into the stream (total head or pressure head) is equal to the sum of the static pressure and the velocity head. The total head pressure is constant along any streamline passing through the airscrew except at the airscrew disc itself, through which the velocity varies continuously, and so the discontinuous change in total head is equal to the change in static pressure. Typical thrust grading curves derived from measurement of change of total head are shown in Fig. 15 below. These curves illustrate the increase of thrust grading produced by the presence of the body, and it appears that the proportionate increase becomes continually greater on proceeding inwards from the airscrew tip. It is the object of the proposed theory to account for this change of thrust and the corresponding change of torque by means of strip theory. The areas under the curves give the resultant thrust on the blades alone; this method is especially valuable when the screw is housed in the body and a direct measurement of airscrew thrust is not possible. The difference between the airscrew thrust and the effective thrust

of the combination of airscrew and body measured on a balance, then determines the drag of the body. In the case of the external screw the integrated thrust was in good agreement with the value measured directly on a balance.

As the increased thrust and torque on a blade element are due to the disturbance produced in the velocity field by the presence of the body, the next stage in the research was to explore the flow around the nose of the body in the absence of the airscrew with a pressure tube yawmeter of the standard type. Results obtained with the bluff-nosed shape (of Fig. 1) are illustrated in Figs. 3 and 4, of which the former shows the observed axial component velocity, and the latter the actual streamline of the flow derived from it by integration. In Fig. 4 the axial component velocity is plotted against radial distance from the axis over a number of planes at right angles to the axis of the body. The position of the planes is indicated in the figure, which includes a section of the portion of the nose included in the exploration. This diagram illustrates the considerable reduction of velocity towards the centre in the plane which would be occupied

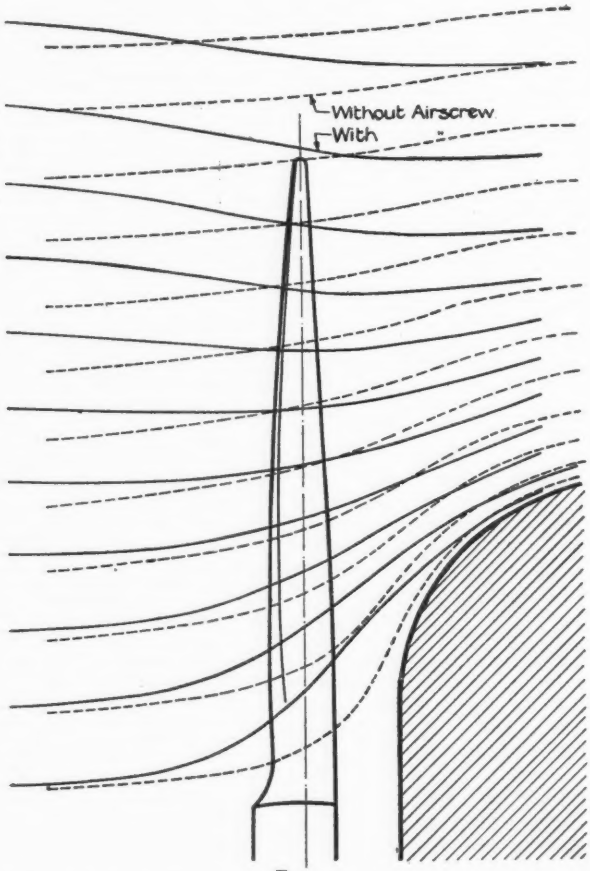

FIG. 4.
Observed streamlines round the nose of body B at $J=.437$ and without screw.

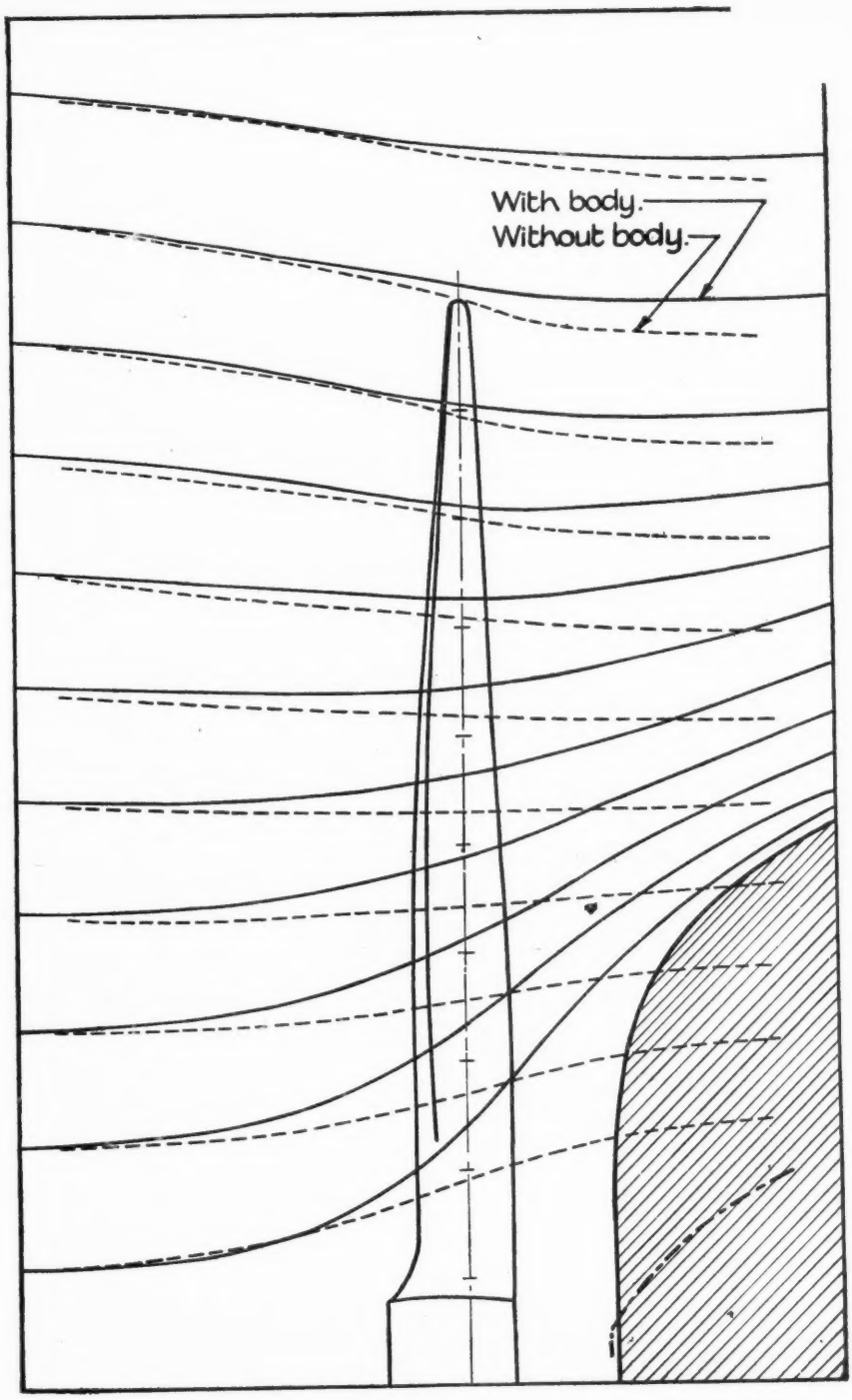

FIG. 5.
Observed streamlines through the airscrew at $J=.437$ with and without body.

by the airscrew; it also shows how the effect may be reversed if the airscrew is placed further back within the nose of the body.

In these earliest experiments the airscrew was outside the body in front, and the initial failure to obtain satisfactory quantitative agreement between theory and experiment may be attributed partly to the effect of the boss of the airscrew and also the bluff and badly streamlined shape of the body. At the time it seemed necessary to investigate further by measuring velocity distribution with the airscrew present and rotating, and also around the same airscrew in the absence of a body. Some of the results obtained are shown here.

Figs. 4 and 5 illustrate the effect of the presence of a body on the observed streamlines through the airscrew when working at a large value of the thrust; Fig. 6, which refers to the same working condition, contains curves of observed axial component velocity plotted against radial distances for different planes normal to the airscrew axis. It illustrates how the effect of the body becomes dominant near the centre, and also the rapid increase of velocity on passing

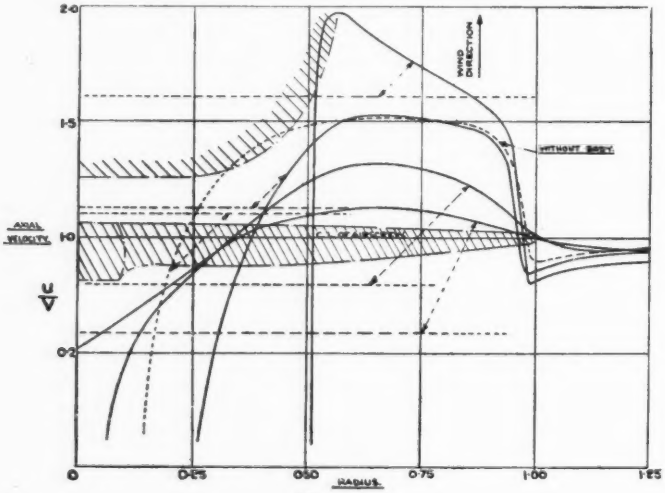

FIG. 6.

Observed axial component velocity in various sections normal to the axis.

through the airscrew disc, which makes it difficult to utilise these observations as a basis for strip theory. In spite of this, the observed velocities in the absence of the body are in general agreement with the values predicted by the vortex theory of airscrews; when the body is present they show a corresponding agreement with the values of axial component velocity deduced from measurements of total head drop through the airscrew. The meaning of this last statement will be more evident after the proposed application of strip theory has been discussed in detail.

4. *Theory*

It is an essential feature of the "Vortex Theory of Airscrews" that the resultant force on the blade element at a given radius depends only on the mean resultant velocity at the airscrew disc at the corresponding radius (see Fig. 7). The condition at the element may therefore be defined by a single variable α (the incidence) which determines the direction of the resultant velocity W. For a given value of the velocity of rotation Ω of the screw, the incidence α determines

directly the value of u (the mean axial component through the airscrew) by the formulæ

$$u/r\Omega = (1 - a_2) \tan \phi \quad [1] \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\phi = \theta - \alpha \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The lift and drag and hence the thrust and torque on the element may then be calculated at once in terms of the incidence α and resultant velocity W given by

$$W = r\Omega\,(1 - a_2) \sec \phi \quad [1] \quad . \quad . \quad . \quad . \quad (3)$$

The resulting formulæ for the elements of thrust and torque are:—

$$dT/dr = bc\rho W^2\,(k_L \cos \phi - k_D \sin \phi) \quad [2] \quad . \quad . \quad . \quad (4)$$

$$(1/r)\,(dQ/dr) = bc\rho W^2\,(k_D \cos \phi + k_L \sin \phi) \quad . \quad . \quad . \quad (5)$$

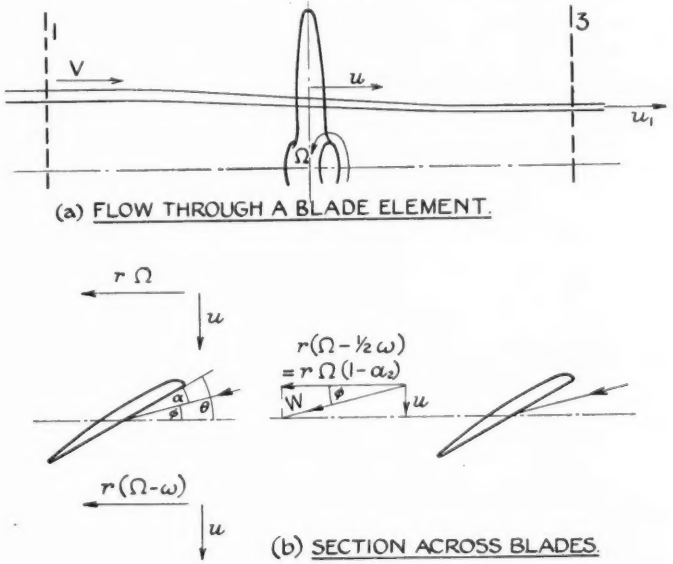

FIG. 7.

These formulæ are all identical in form with those of primitive strip theory without allowance for inflow, except that u replaces V the velocity of advance, the small rotational inflow factor a_2 is added, and k_L and k_D are the lift and drag coefficients of the section in two dimensional flow, or the values obtained experimentally on a model aerofoil of finite aspect ratio, reduced to infinite aspect ratio by the standard formulæ of the Prandtl theory. It remains only to determine the value of the velocity of advance V of the airscrew corresponding to the assumed conditions at the blade element; the resulting formula which is derived below may be written in the form

$$V/u = \{\, 1 - (1/4\pi\rho r u^2) \,\}\,(dT/dr) \quad . \quad . \quad . \quad . \quad (7)$$

$$= 1 - F \text{ (definition of } F) \text{ [3]}$$

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[1] a_2 is a small rotational inflow factor which is determined from considerations of angular momentum which lead to the equation:—

$$a_2 = (1/4\pi\rho u \Omega r^3)\,(dQ/dr) \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

from which a_2 may be determined with the help of equations 1, 5 and 3.

[2] See Table of Notation.

[3] The more familiar form of this relation is expressed in terms of the inflow factor $a = (u - V)/V$, and the thrust coefficient of the type $(1/4\pi r\rho V^2)\,(dT/dr)$, when it takes the form

$$(1/4\pi r\rho V^2)\,(dT/dr) = a\,(1 + a).$$

The present arrangement is more convenient in computation and in the development of the theory.

This formula (with equation (1)) determines the value of the ratio $V/R\Omega$ corresponding to any assumed value of a at the given element; in practice it is necessary to perform the inverse operation of determining a, dT/dr, dQ/dr, etc., at a sufficient number of radii, for given values of $V/R\Omega$. This may be done by making the calculations for a sufficient number of assumed values of a and cross plotting.

The essential point of the present theory of airscrew body interference is that the presence of the body modifies the axial component velocity at the airscrew disc only, or conversely that the velocity of advance, corresponding to given conditions at the blade element, is modified by the presence of the body. In fact, all the above equations still hold with the exception of equation (7). It will therefore be convenient to discuss in succession the derivation of equation (7) and of the correspondnig equation with body interference.

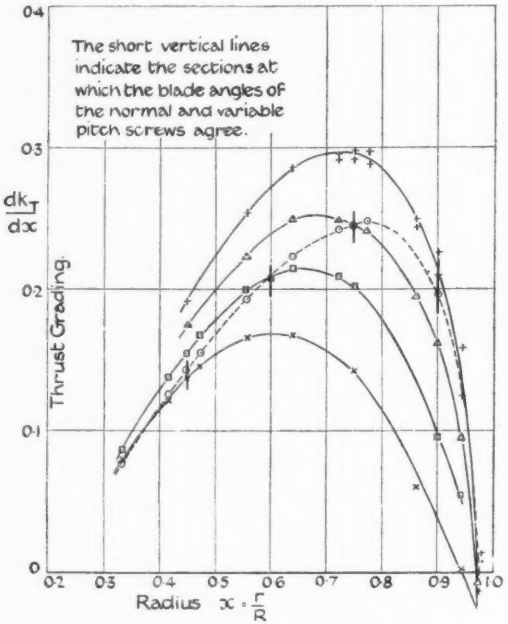

FIG. 8.

Experimental verification of the independence of neighbouring blade elements.

In the vortex theory equation (7) is derived in part from considerations relating to the trailing vortices shed by the blades, but the form of the resulting equation is identical with that originally derived by Froude from considerations of momentum and energy. The only limitation is that strictly the equation of momentum should apply to the whole screw, while equation (7) must be derived separately for each radius; in this respect the original treatment of the vortex theory is superior. Equation (7) may, however, be derived from Froude's relation for the whole airscrew with the aid of the additional assumption of the " independence of neighbouring elements " (at different radii) which is evidently inherent in equation (7) since it applies to a definite radius r.

It may be worth noting here that this independence has been checked experimentally by measuring the total head drop for pairs of airscrews having the same section and blade angle at a standard radius r, but differing at other radii. If the pair of airscrews is run at the same forward and rotational speeds, the value of dT/dr according to the above equations should be identical at the standard radius. The observed agreement of the value of dT/dr deduced from total head drop is shown in Fig. 8 for four pairs of screws, the standard radius being marked by a short vertical line. The agreement is very good when the standard radius is at $.6R$ and $.75R$, but less so near the tip and centre.

The derivation of the axial inflow relation corresponding to (7) may now be discussed with reference to Fig. 9. Assume that the tractor body has a substantial parallel portion opposite which a section (3) may be found in which the flow is parallel to the axis, and the pressure has regained its atmospheric value. The variation of pressure head along a typical stream tube will be unaffected by the presence of the body; it will be constant from section 1 to a point just in front of the airscrew, will change by the amount $(1/2\pi r)\,(dT/dr)$ on passing through the airscrew, and will then remain constant as far as section 3. The resulting relation is

$$(1/2\pi r)\,(dT/dr)=\tfrac{1}{2}\rho\,(u_1{}^2-V^2)$$

or

$$dT=2\pi r dr\times\tfrac{1}{2}\rho\,(u_1{}^2-V^2) \quad . \quad . \quad . \quad . \quad . \quad (8)$$

and its form may be taken to be independent of the presence of the body, assuming the latter to be of easy shape. In writing down the overall momentum relation, however, it is necessary to include any resultant force on the nose of the body which may arise from increase of pressure due to the screw. Assuming that the resultant pressure on the nose is zero in the absence of the screw which is roughly true of a number of shapes, then the resultant P of the pressure over the whole nose with airscrew running may be inserted in the momentum relation in the form

$T-P=$ difference of rate of flow of momentum across sections 1 and 3

$$=\int_0^R \rho u\,(u_1{}'-V')\,2\pi r dr\ [4] \quad . \quad . \quad . \quad . \quad . \quad (9)$$

The pressure P is zero in the absence of the body, and by virtue of the independence of neighbouring elements the corresponding equation

$$dT=\rho u\,(u_1-V)\,2\pi r dr \quad . \quad . \quad . \quad . \quad . \quad (10)$$

holds for each element separately. The result of eliminating u_1 between equations (8) and (10) gives equation (7). When the body is present the equation corresponding to (9) and (10)

$$dT-dP=\rho u\,(u_1{}'-V')\,2\pi r dr \quad . \quad . \quad . \quad . \quad . \quad (11)$$

will be taken to define an "element of pressure" dP, the definition being consistent with the relation

$$\int dP=P \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

by virtue of (9).

The sole object of defining dP is to make the important assumption that dP is proportional to dT so that it is possible to write

$$dP=h\,dT \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

where h is a function of the radius r only. This assumption is a natural extension of the experimental fact that the drag coefficient of a body with airscrew running is a linear function of the thrust coefficient. Substituting for dP from (13) in (11) and eliminating u_1 between (11) and (8) gives

$$V/u=(1/1-h)-(1-h)\,F\ [5] \quad . \quad . \quad . \quad . \quad (14)$$

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[4] Here and in Fig. 9 dashes are used to distinguish quantities altered by the presence of the body.

[5] The corresponding formula in the more usual notation is

$$1/4\pi r\rho V^2\,dT/dr=(a+h)\,(1+a/(1-h)^2$$

which reduces to (7) when h is zero. By this equation in combination with equations (1) to (6) which are unaltered by the presence of the body, the problem of calculating the performance of any airscrew under the influence of the given body, together with the value of P, is reduced to the determination of a single parameter h which is a function of position relative to the nose of the body. (It is now assumed that h is not only independent of the working condition, but also of the design of screw and its diameter.) It is of interest to notice that the result of putting $F=0$ in (14) is

$$h=1-u/V$$

This value of u/V corresponds to zero thrust on the element and may therefore be reasonably assumed to be equal to the value at the corresponding point in the absence of the screw. This further supports the suggestion that the value of h is independent of the type of screw.

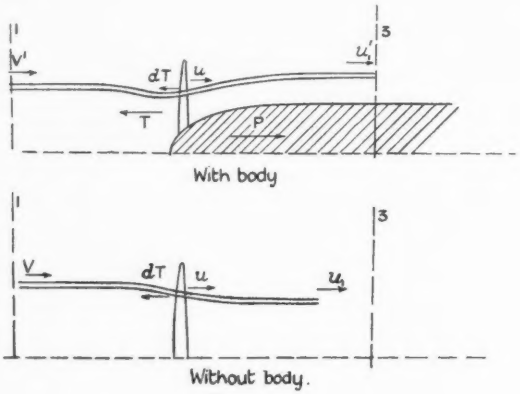


FIG. 9.

Flow through a blade element with and without a tractor body.

Actually an equation similar to (14) was first derived on purely empirical grounds from the analysis of observed thrust grading curves by the method about to be described. Fig. 18 illustrates the effect of the body on the thrust grading curve and the corresponding value of u/V or h plotted against radius.

For any screw whose performance has been calculated by the vortex theory, values of V/u, F and dT/dr (for given Ω) will be available for a range of values of a and r; V/u and F may then be plotted against dT/dr for given r and Ω. Hence, if values of dT/dr are available from observations of total head drop,[6] values of V/u and F may be deduced and inserted in equation (14) which may then be solved for h.[7] A value for h is thus obtained for each radius corresponding to each working condition at which observations of total head are available. The degree of constancy of h is illustrated in Figs. 10 and 11 for a typical case. Fig. 10 shows values of V/u plotted against F, both being derived from observations of total head; the straight lines are given by equation (14) with values of h as shown. The agreement is good except for the most central section at radius $r=.3R$. Fig. 11 compares recalculated values of thrust grading

[6] The method assumes that the sections are all working at angles of incidence below the stall; when the section is stalled the method may give ambiguous results or fail to give a solution.

[7] Equation (14) is a quadratic equation in h of which the two roots are

$$h=1/2F\ \{(1+2\ F-G)\pm[(1+2\ F-G)^2-4F\ (G-F)]^{\frac{1}{2}}\}$$

where G is written for $1-V/u$, and the lower sign is to be taken because it makes h vanish for $G=F$.

(points) with observation curves. The agreement is seen to be sufficiently close for the points shown.[8]

The process of recalculating the performance of the screw when h is known is even simpler. Given the data for the blade elements it is only necessary to multiply the value of V/u or $J\,(=V/nD)$ in the absence of the body by the factor $\{1/(1-h)-(1-h)F\} \div (1-F)$ to obtain the corresponding value of J at a given radius when the body is present. The process of cross plotting to obtain the thrust and torque grading for given J is then performed exactly as in the absence of the body.

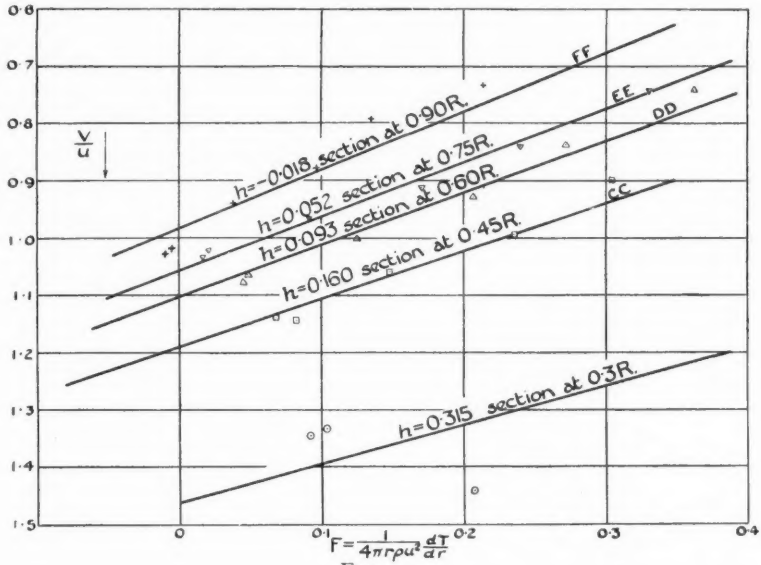


FIG. 10.

Analysis of thrust grading curves for the airscrew in forward position, blunt nosed body (Fig. 2).

This method when applied to the torque completely solves the problem of determining the effect of body interference on the power for given revolutions if the value of h is known; it could also be used to modify the design of screw slightly to give the same revolutions for the same power. The corresponding evaluation of the thrust on the blades is of little practical importance without the theory of body drag developed below.

5. *Efficiency*

According to the above analysis the flow as far as the maximum section of a streamline body with tractor airscrew does not involve any source of energy loss not present when the screw is working alone. The resultant pressure P on the nose is the same as the pressure drag defined in the introduction as corresponding to the change in airscrew thrust due to body interference, the resultant force $T-P$ is an "effective" thrust representing the whole of the *mutual* inter-

[8] The section at radius $.3R$ appears from experiments on the aerofoil section to be installed for $J=.623$ and under, and observed values of dk_T/dx at $J=.623$, .529 and .437 are higher than the observed maximum lift of the section.

ference of aiscrew and body since the remainder of the increase of body drag would exist if the screw were so far in front as to be outside the influence of the body. It seems therefore that a reasonable expression for the useful work done by the screw would be $V(T-P)$ which should yield an "effective" efficiency differing but little from the efficiency of the screw working alone. The theoretical efficiency will first be compared for elements of the screw at a single radius and for a given value of the conditions at the element; in practice the nearest approach to this ideal is to make the comparison at a given value of the torque coefficienct k_Q.

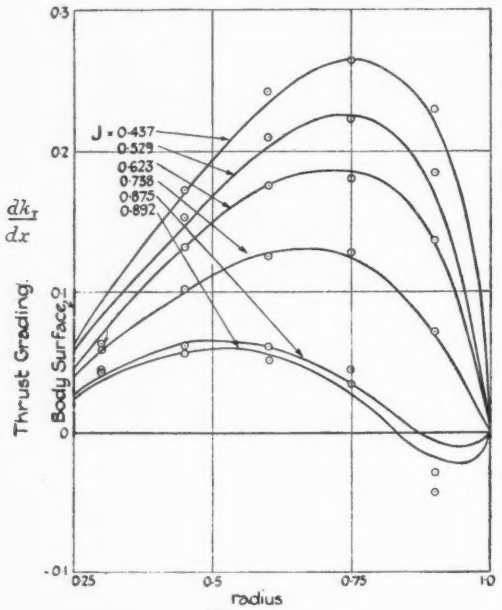


FIG. 11.

Comparison of observed thrust grading curves with points recalculated from the mean values of h. (Same conditions as Fig. 10).

The energy wasted by an airscrew working alone may be conveniently divided into the sum of three parts for an element at a given radius: (A) the energy in the axial component velocity in the wake, (B) the energy in the rotational velocity in the wake, and (C) the energy lost by the profile drag of the blade elements. Reckoned in units of power it may be shown that

$$A = \tfrac{1}{2}\rho\,(u_1 - V)^2\, 2\pi r_1 dr_1 u_1 = \tfrac{1}{2}\rho\,(u_1 - V)^2\, u\, 2\pi r dr$$
$$B = \tfrac{1}{2}\rho r^2\omega^2 2\pi r_1 dr_1 u_1 = \tfrac{1}{2}\rho r^2\omega^2 u\, 2\pi r dr$$
$$C = bc\rho W^3 k_D dr$$

When the screw is working alone the conservation of energy requires that

$$\Omega dQ - V dT = A + B + C,$$

a relation which may be easily verified. When a tractor body is present the corresponding equation is

$$\Omega dQ - V\,(dT - dP) = A + B + C$$

which may be similarly verified from equations (11), (8) and (1)—(6). Comparing corresponding values with and without body (see Fig. 9) for given conditions at

the blade element and distinguishing the former for the moment by dashes so that

$$dQ' = dQ,\ dT' = dT,\ \text{etc.},$$

it can be shown at once that

$$B' = B \text{ and } C' = C$$

but

$$(u_1' - V) = (1 - h)(u_1 - V)$$

so that

$$A' = (1 - h)^2 A$$

In the normal case therefore where dP and h are positive, u_1' is always less than u_1 and C' is less than C and so the "effective" energy loss is reduced by the influence of the body *for given conditions at the blade element*. The effect is fairly small in practice, and it was found in a typical case that the result of taking values of A, B and C at a particular working condition, multiplying A by the factor $(1 - h)^2$ and integrating along the blade was to give an increase of

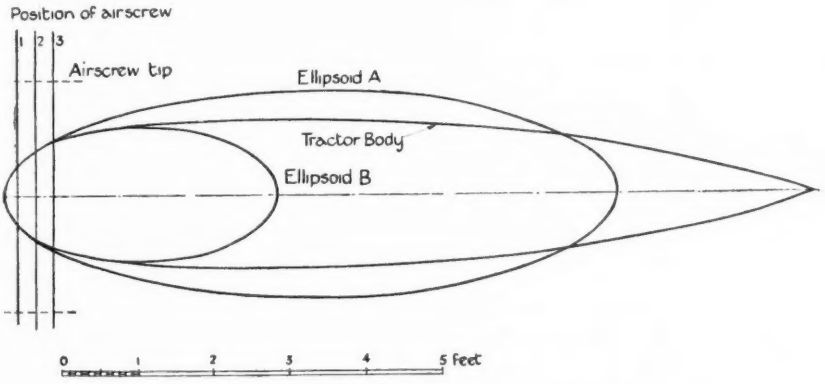


FIG. 12.

"effective" efficiency of 2.6 per cent. Actually this method is not correct, since for a given overall torque coefficient the conditions at the different radii cannot all be identical with and without body so that B and C will be altered as well as A. An exact calculation showed that in the typical case at maximum efficiency the above figure of 2.6 per cent. must be replaced by 0.3 per cent.

The practical conclusion seems to be that the efficiency is certainly not reduced by housing the airscrew in the nose by means of a spinner, as compared with the value that would be obtained by putting the screw far enough in front to be outside the influence of the body but leaving the body in the slipstream.

6. *Theoretical Evaluation of the Parameter h*

It has already been noticed that the parameter h is equal to the value of $1 - u/V$ at the blade element for zero thrust and may therefore be assumed equal to the value of the same quantity at the same point in the absence of the airscrew. For certain simple shapes such as the prolate spheroid (ovary ellipsoid) moving parallel to the axis of symmetry, the velocity distribution in a perfect fluid is well known, and the actual distribution round the forward half in the

absence of the screw is believed to be not greatly different. For the blunt nosed streamline body, shown in Figs. 2 and 12, an attempt has been made to calculate the velocity field by fitting spheroids to the nose. The two spheroids shown both have close contact with the section of the body at the nose; spheroid A fits most closely near the nose, while spheroid B is more nearly of the correct volume. Curves of u/V against ω (normal distance from the axis) have been calculated for both spheroids for the three sections shown in the figure for which values of h for the body have been calculated from the analysis of thrust grading curves on an airscrew. The Fig. 13 shows that the values of h are intermediate between the curves of $1-u/V$ for the two spheroids which diverge from each other most at the aftermost section; but are still sufficiently close together to serve as a satisfactory basis for deducing the performances of the airscrew.

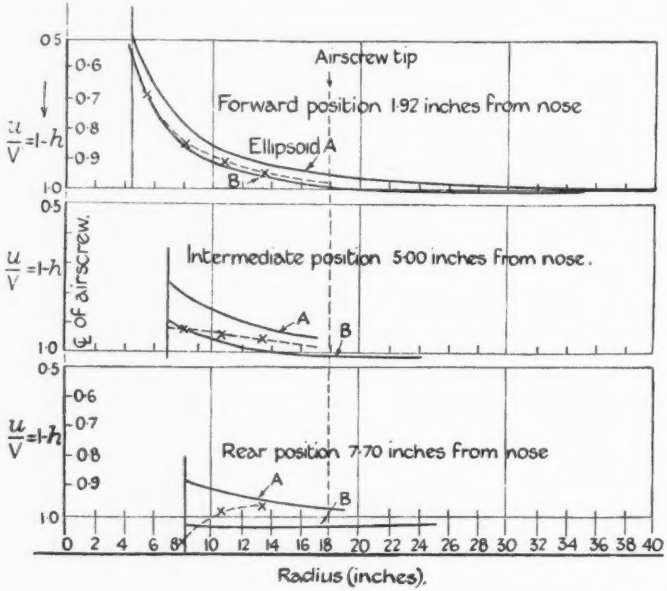


FIG. 13.

Calculated values of axial component velocity for the two ellipsoids compared with the value of $1-h$ (dotted lines).

An interesting minor point in connection with these curves, is that by the condition of continuity the same total quantity of fluid must flow across any plane normal to the axis. Hence when u/V is less than unity at small radii as in Fig. 13 (forward position), it must be greater than unity at a sufficiently large radius. The actual values can be calculated for the ellipsoids and it appears that the value of u/V rises to unity at radii 61 inches and 23 inches for ellipsoids A and B and has maximum values of 1.002 and 1.006 in these two cases respectively. In calculating the effect on the airscrew it is, therefore, sufficiently accurate to assume that the value of u/V tends continuously to unity with increasing radius.

For the pointed nose, more usual in practice, the theoretical calculation would present greater difficulties.

7. *General Discussion of the Agreement of Theory with Experiment*

The most complete analysis that has so far been made by this theory refers to a 4-bladed tractor airscrew housed in three different positions within the nose of a streamline body, see Fig. 2. Two alternative shapes of nose were used which were respectively blunt and pointed, thus making six cases in all. The Figs. 14*a* and *b* show the comparison of recalculated thrust and torque coeffi-

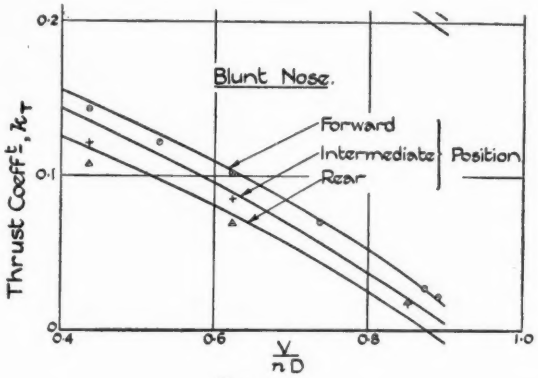


FIG. 14*a*.

cients with observations for the blunt nose, the calculated values being based on the single function of position h already defined as the retarding effect of the body on the fluid at each point for all working conditions of the airscrew. As this function is itself derived from the observed thrust grading curves, the agreement between observation and calculation on thrust only serves to verify the independence of working condition; for the torque the agreement represents a real verification of the blade element theory. The function representing the retarding effect of the body is assumed to be applicable to screws of any design or diameter, but so far no variations of this kind have been tried with this particular type of body.

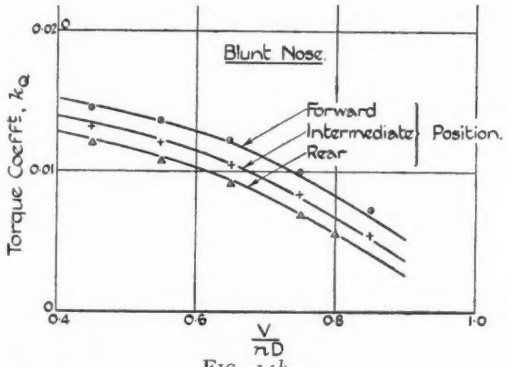


FIG. 14*b*.

Comparison between observed airscrew thrust and torque (points) and calculated values (full lines).

8. *Effect of Diameter or Design of Screw*

The only case in which the relative dimensions of body and airscrew have been varied while keeping the shapes unaltered is that of the bluff-nosed body with external screw illustrated in Fig. 1. Fig. 15 shows how the thrust grading curves for screws of different relative diameters may be deduced from the same curve of h (axial component velocity). Actually the velocity curve was deduced from the observed thrust grading curve with the smaller airscrew radius 1.67 times maximum body radius; the thrust grading curve for the larger airscrew

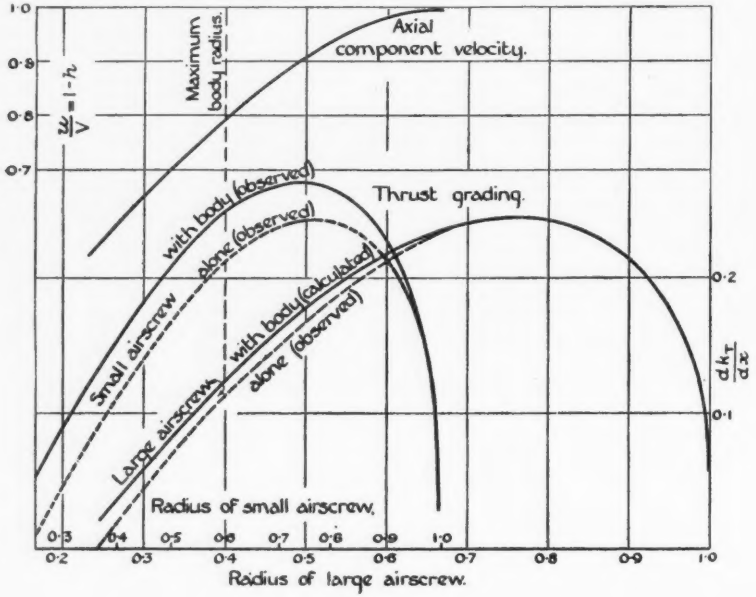
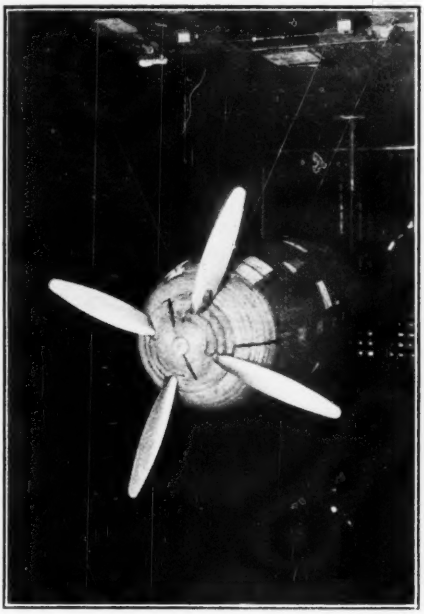


FIG. 15.

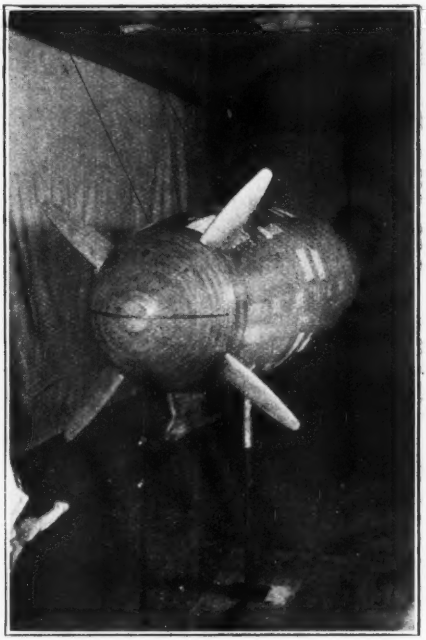
Effect of varying relative size of body and airscrew.

(2.5 times maximum body radius) was then deduced from it. The overall thrust only was observed for the relatively larger airscrew; it was compared with the value deduced by integrating the theoretical thrust grading curve, and the agreement was found to be good. The larger diameter screw is of the relative size usual in practice, and the good agreement suggests that results deduced by the theory from model experiments on a relatively large diameter body may be applied with confidence to the relatively smaller body usual in actual machines. A comparison was also made of thrust curves for similar screws having two and four blades; the value of h was deduced by giving equal weight to both screws and the results were very fairly consistent.

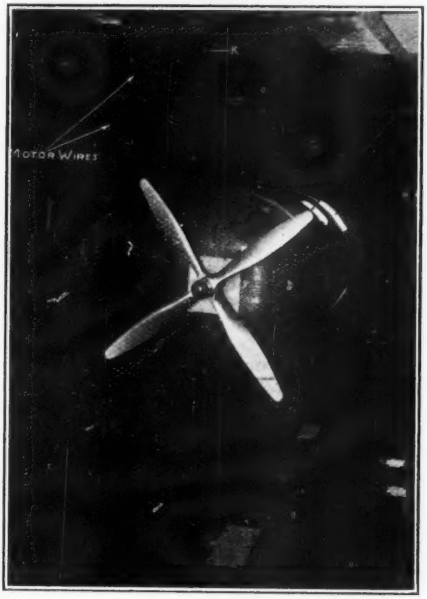
These screws have the value of geometrical pitch ratio 0.7. The only experimental data available as to the effect of change of pitch refers to a four-bladed screw of the high pitch ratio 1.5. The theory appears to break down for this screw, but as the vortex theory also failed for this high value of pitch, little importance is attached to the failure, as it seems likely that the theory will hold good for the range of pitch usual in practical airscrew design. As already mentioned, the theory is not very satisfactory in its application to this type of



(A) *Blunt body of Fig. 2 with screw in forward position.*



(B) *Blunt body of Fig. 2 with screw in rear position.*



(C) *Body B of Fig. 1.*

FIG. 16.

body with blunt nose and external screw; with the streamline body and spinner the theory should be applicable over a greater range of airscrew diameter.

9. *Pressure Plotting Body with Screw Running*

The subject of the resultant pressure drag on the body suggests an additional experimental check on the theory, viz., to pressure plot the body with airscrew running. This experiment has been carried out quite recently, and has verified the existence of a resultant pressure drag whose magnitude agrees sufficiently closely with the value predicted. The next few figures illustrate these experiments. The blunt tractor body of Fig. 2 with the screw in the forward position

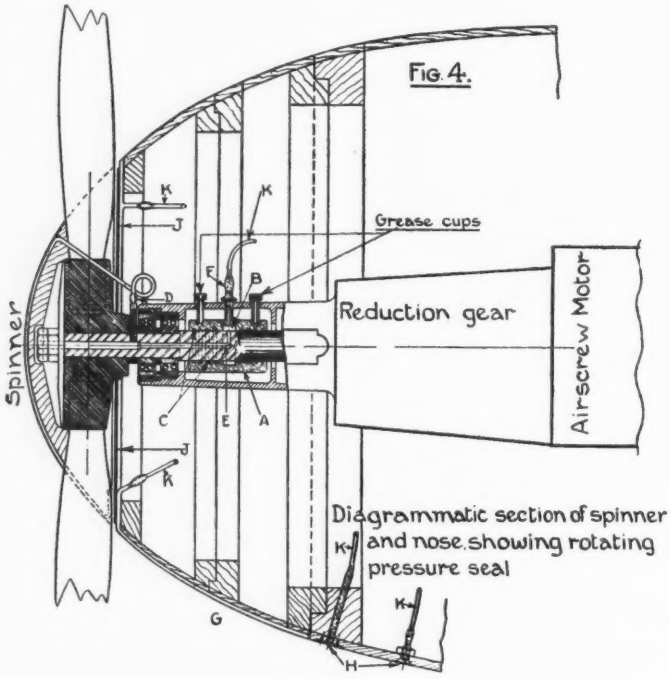


Fig. 17.

(Fig. 16A) was chosen for the first experiment, as it gave the largest resultant pressure drag. The same screw in the after position is shown in Fig. 16A for comparison, while Fig. 16C shows the same screw mounted externally to the bluff-nosed body C of Fig. 1. The surface of the body (Fig. 16A) included a rotating spinner of appreciable radius, and a rotating seal (Fig. 17) was necessary to transmit pressures from its surface to the fixed manometer. It was found that the pressure at a given point of the surface of the spinner was steady, but showed large variations round the circle, so that a large number of pressure holes were required. The pressure tubes were arranged as arcs of circles of seven different radii in addition to one radial tube (Fig. 16A); each circumferential tube had sufficient holes to define a curve of pressure over a quadrant, the screw having four blades.

These experiments provided an opportunity for separating the airscrew thrust and body drag by a more direct method than that of deducing the airscrew thrust from difference of total head pressure as previously described. The arrangement of balances for measuring thrust, torque and body drag is shown in Fig. 18. The thrust is transmitted to the thrust balance through the inclined wire FG, and a known torque can be applied by transferring weights from one to the other of the scale pans K. This arrangement has the advantage that there are no objects other than wires in a position to cause interference on the body drag. To obtain the total body drag it was necessary to add to the reading of the drag balance, the drag of the spinner deduced by integrating the pressure measure-

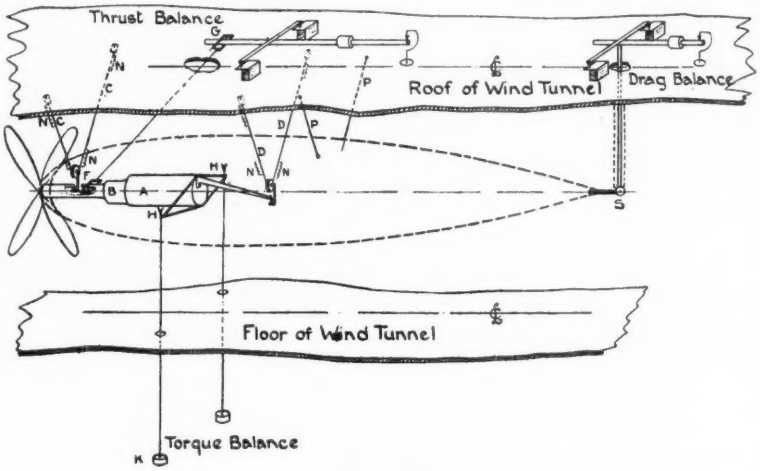


FIG. 18.

Arrangement of balances for measuring thrust torque and body drag.

ments. For this purpose it was necessary to observe the pressure distribution over a plate (J, Fig. 17) forming the rear boundary of the gap between spinner and body. The same correction applied to the thrust balance reading determined the thrust of the blades alone. Values of the total body drag obtained by the old and new methods are distinguished in Fig. 20 by the letters *a* and *b* respectively. Fig. 19 shows the variation of pressure along the nose of the body at a large value of the thrust and also with the screw removed. The change of pressure coefficient due to the airscrew was roughly proportional to the thrust coefficient $T_c = T/\rho V^2 D^2$. The curves show the large increase of pressure with thrust immediately behind the screw which gives rise to the pressure drag on the nose as discussed in the theory. In Fig. 20 the corresponding drag coefficient is plotted against thrust coefficient, the different lines representing respectively the pressure drag on the forward half only, the resultant drag due to pressure over the whole body and the total drag as measured on a balance, the difference between the second and third representing the skin friction. The pressure drag on the forward half is compared with the value which was deduced from the thrust grading curves according to the present theory before the pressure measurements were undertaken and the agreement is sufficiently good to justify the theory.

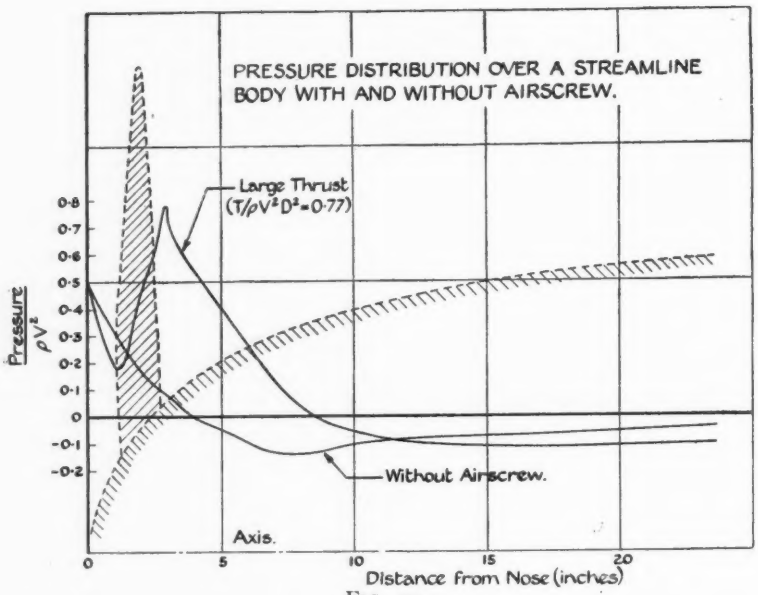


FIG. 19.

Pressure distribution over a streamline body with and without airscrew.

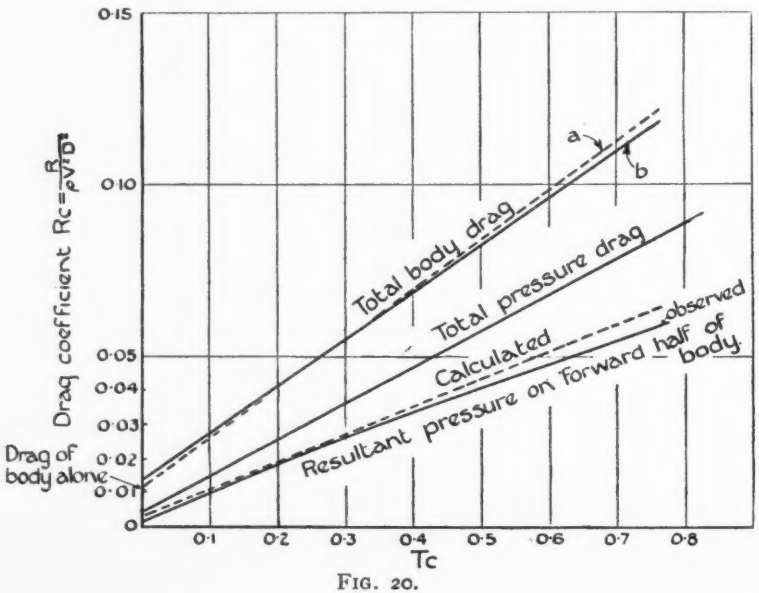


FIG. 20.

10. *Effect of Excrescences*

The discussion of the theory is now complete as regards the mutual interference proper which includes the pressure drag and the change of airscrew performance. For practical purposes it is necessary to deal with the whole body drag. This may be separated into the parts defined in the introduction of pressure drag, velocity drag and turbulence drag. The pressure drag which is now identified experimentally and theoretically with the resultant P of the observed pressure over the whole front half of the body, may also be tentatively identified with the difference between the actual body drag and the value it would have if the screw were placed at a sufficient distance upstream to be uninfluenced by the body. The remaining body drag may be separated into

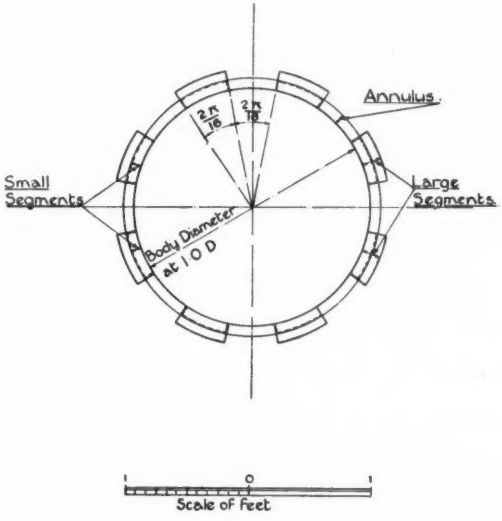


FIG. 21.

Excrescences on the blunt body of Fig. 2 in the plane 1.0 *diameters behind the airscrew in the forward position.*

velocity drag R_v and turbulence R_t [9] of which the former is defined to be equal to the drag on the body alone increased in the ratio (u_1^2/V^2) of the square of the mean slipstream velocity to the square of the velocity of advance. Equation (8), which is unaffected by the presence of the body, can be expressed in the form

$$(u_1^2 - V^2)/V^2) = (1/\pi r \rho V^2)\,(dT/dr)$$

which given for a "mean square" value of u_1

$$u_1^2/V^2 = 1 + 2T/\pi R^2 \rho V^2 = 1 + (8/\pi)\,Tc = 1 + 2.5 \ldots Tc.$$

This equation furnishes the explanation of why the drag coefficient is almost universally a linear function of the thrust coefficient. It is obviously not possible to separate the drag of the body

$$R = P + R_v + R_t$$

into its component parts with any definiteness, but the idea of such a separation is useful in connection with certain additional experiments. The experiments

[9] A part of the turbulence drag may be due to the direct effect of centrifugal force on the rotating slipstream, but this effect is probably very small in practice.

already described relate to the streamline body with spinner, while the drag of an actual aeroplane body is usually greatly increased by the presence of excrescences such as cockpits, wind shields, etc. The present discussion is confined to excrescences sufficiently far behind the screw to leave its performance and therefore presumably the pressure drag P unaltered. The increased drag, due to excrescences, was imitated experimentally by adding projecting rings and segments to the body as shown in Fig. 21. The amount of projection was varied to give a drag varying from 2 to $4\frac{1}{2}$ times the drag of the bare streamline body without airscrew. It has already been argued that the pressure drag P with screw running is unaffected by excrescences, but it is to be expected that the combined velocity and turbulence drag which make up the whole drag of the body alone will for a given thrust vary with the number of excrescences in the same ratio as the drag without screw. This should be definitely true of the velocity drag, but as regards the turbulence drag it is to be expected that the

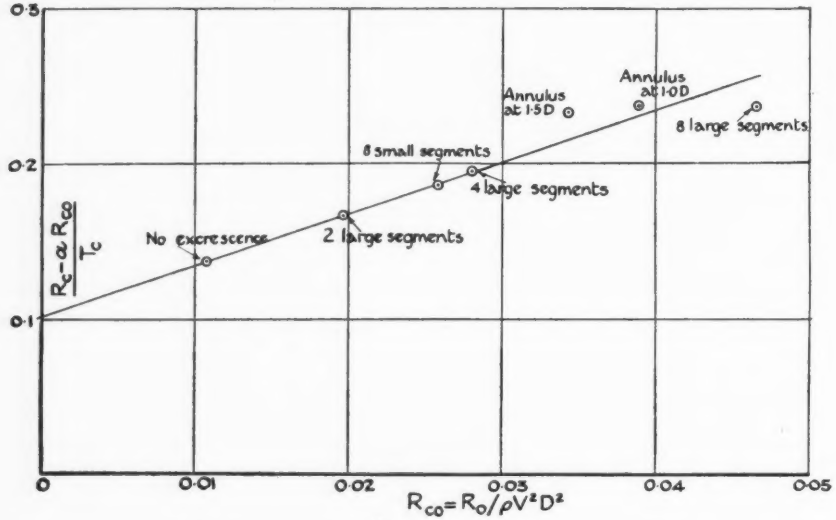


FIG. 22.

Analysis of experiments with excrescences.

spoiling effect may be relatively greater for the bare body and might as an extreme case be almost independent of the excrescences. This argument suggests the following method of analysing the observations of drag and airscrew thrust with variable excrescences. For a given thrust plot the drag R of the bodies with excrescences against the drag R_0 without screw, each point representing a different number of excrescences (Fig. 22). The resulting curve should be a straight line of which the ordinate for zero R_0 (obtained by extrapolation) should be equal to the part of the drag independent of excrescences (roughly the pressure P in the nose), while the slope should represent the ratio in which the velocity and turbulence drag is increased by the increased slipstream velocity at the given thrust.

Actually since the drag coefficient on a velocity base is a linear function of the thrust, it is possible to use the slope of this curve in place of the actual drag as ordinate in the figure, so that each point represents the result of observations over the whole range of thrust. The actual formulæ are as follows:—

The total drag coefficient

$$R_c = R/\rho V^2 D^2$$

is a function of thrust coefficient T_c and number of excrescences which may be represented by the drag coefficient R_{co} of the body alone. Then

$$R_c = aR_{co} + b_1 T_c + b_2 R_{co} T_c$$

where b_1, b_2 are independent both of T_c and R_{co}. $b_1 T_c$ represents the part of R_c independent of excrescences. Fig. 22 represents

$$(R_c - aR_{co})/T_c$$

plotted against R_{co}, its equation being

$$(R_c - aR_{co})/T_c = b_1 + b_2 R_{co}.$$

The actual values obtained from Fig. 22 make the constant term b_1 somewhat larger than the pressure drag which is consistent with the suggestion that the turbulence effect is somewhat larger for the bare body. The mean value of a appears to be slightly greater than unity, representing a slight spoiling effect at zero thrust.

The fact that the coefficient b_2 has a value (between 4 and 5) greater than the theoretical value $8/\pi$ for the velocity drag indicates that the ratio of turbulence drag to velocity drag has a value a little less than unity for the bare body; the turbulence drag may also probably be identified with the increase of suction on the tail above the value corresponding to the increase of slipstream velocity, as observed in the pressure plotting experiments. The skin friction drag deduced from these experiments appeared to be little greater than would be expected from the increase of velocity.

11. *Practical Applications*

It is now possible to present a practical method for design purposes stripped of complications which were only required in the development of the theory. It is first assumed that for the shape of nose of the particular fuselage under discussion the function h is known either from observations of thrust grading on a particular model airscrew, or by the method of calculating the flow in a perfect fluid. Only extremely rough values of h would be required in a practical case, but on the other hand it was found that h changes very rapidly with the position of the screw along the axis (Fig. 13). From the value of h the torque coefficient of any screw can be calculated with comparatively little trouble if an analysis by the vortex theory of the performance of the screw working alone is available. This solves the problem of the effect of the presence of the body on the engine power required to produce a given rotational speed. Next calculate an " effective " thrust

$$T_2 = T - P$$

from the assumption that the effective efficiency for a given torque coefficient is unaltered by the presence of the body (section 5). The net thrust T' of the body and airscrew combined is then given by

$$T' = T_2 - R_2$$

where R_2 is the slipstream drag and its coefficient is given by the equation

$$R_2/R_0 = 1 + b \,.\, T_{c2}$$ [10]

The value of b appears from experiments to be independent of the extent and sign of the mutual interference of the airscrew and body, and a suitable mean value is about 5.0 or nearly double the value required by the theoretical slipstream velocity, but its value may be appreciably greater in certain cases.

---

[10] The value unity of the constant term in this equation is fairly well borne out by the experiments with streamline body and spinner, but evidently requires that the shape of the nose should be continued accurately through the screw. The fact that the constant term was appreciably greater than unity with the bluff nosed body and external screw is sufficiently accounted for by the interference of the airscrew boss even at zero thrust.

Wherever the effect of body interference on the torque may be neglected the present method reduces to the standard one of allowing for the effect of the slipstream in increasing the body drag, provided that the coefficient b is increased to the value just mentioned.

12. *Pusher Airscrews*

It is interesting to speculate on the application of the present theory to pusher screws. If a reasonably streamline flow occurred along the body up to and through the position of the screw, as might be the case for a really good shape, the previous argument suggesting that the efficiency, reckoning the airscrew thrust and pressure drag in the useful work, would be practically unaltered, would still seem to apply, while the slipstream effect is practically absent. It would appear, therefore, that the overall efficiency of the combination should be little less than that of the body and screw working independently, and might even be greater.

The only pusher experiments at present available were made with the high drag forms shown in Fig. 1, having an extremely bluff surface just in front of the screw. Experiments with pusher screws housed in different positions within the tail of a body, fairly short indeed, but having quite low drag, are to be commenced in the near future at the National Physical Laboratory. It is proposed to attempt to analyse the experiments on the lines of the present theory. It might happen in certain cases that an airscrew housed in the tail of a badly streamlined body would actually improve the performance by delaying the breakaway of the boundary layer. Its action might be analogous in this respect to that of a wind tunnel fan at the exit of an expanding cone. Another point of view is that the breakaway of the boundary layer is believed always to occur behind the point of minimum pressure; the suction of a pusher screw might cause the surface pressure to decrease continuously up to the front of the screw.

13. *Conclusions*

We are now in a position to give tentative answers to some of the questions asked at the outset, though the answers may not seem very satisfactory. Is a blunt spinner better than a pointed spinner to a streamline body? The answer seems to be that it makes very little difference, the blunt nose having a slight superiority when the screw is far back. As regards the best position for housing an airscrew in the nose, it appears that the most forward position in which the boss and inefficient part of the blade roots is just covered by the spinner is slightly the most efficient, and this is also the position likely to be chosen for practical reasons. In this position the interference effect on the torque is less with a pointed spinner than it is with a blunt spinner covering the same area of the airscrew disc, which may account for the preference apparently given to the pointed shape.

The method of calculation by strip theory already described provides a complete solution to the problem of determining the effect of the body on the torque coefficient of any airscrew, or the power required for a given rotational speed; the special data required as a basis for the calculation depend only on the external shape of the nose, but may be difficult to obtain in particular cases. When the nose approximates to an ellipsoidal form the theoretical method may be applied; for a pointed nose the empirical results given in R. & M. 1120 may suffice.

Fig. 23 gives some answer to the question of how closely the model experiments are applicable to practical forms of body. They show outlines (traced from photographs in " Flight " and idealised) of three recent types of fuselage having spinners. Apart from cylinder heads, etc., the shapes are not vastly different from those employed in the model experiments; the screw is somewhat

smaller in relation to the body diameter in the model, but not out of all proportion.

The experiments suggest that the action of a tractor screw housed in the nose of a streamline body does not greatly spoil its shape, but other and better shapes must be tried before this conclusion is accepted as final. If this is also true of a pusher screw, then the pusher screw should be aerodynamically superior to a tractor. The experiments suggest that the loss of efficiency due to inter-

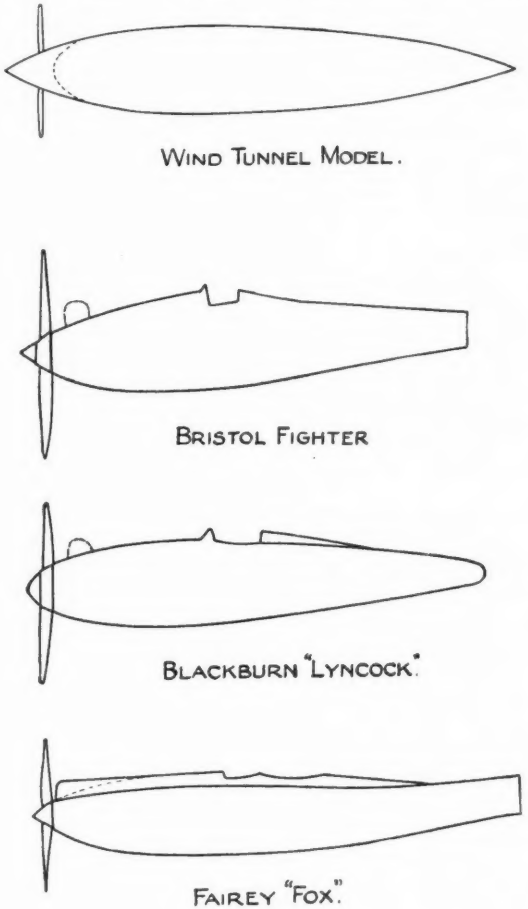


FIG. 23.

ference is sufficiently accounted for by the well-known formula which assumes that the increase of drag is due to slipstream velocity and is proportional to the thrust, the constant of proportionality being increased somewhat in the case of a good streamline form to allow for the spoiling effect of the airscrew. It is more difficult to generalise as to the effect of interference on the torque, since the effect varies critically with the shape of the nose and position of the screw in it. For a type in which a bluff radiator is present close behind the screw,

the results of experiments with the original bluff-nosed tractor bodies may be applied, but when a nose of easy shape is continued through the screw by a spinner the effect is much smaller and may even reduce the torque instead of increasing it. As regards the best relative size of airscrew and body, the experiments do not suggest that when the screw is housed in the body there is much loss of efficiency as a result of increasing the diameter of the spinner to nearly one-half the diameter of the airscrew. The earlier experiments on three different sizes of body indicate that the proportional increase of body drag for a given airscrew thrust coefficient is fairly independent of the relative diameter of the screw and body. The slipstream drag will, of course, be reduced by increasing the diameter of screw since the slipstream velocity for given thrust will be smaller; the improvement will be additional to that expected on the airscrew alone. Finally, the experiments with excrescences suggest that when the ideal streamline body is spoilt by the addition of cockpit, undercarriage, etc., their addition to the interference may be accounted for on the basis of the increase of slipstream velocity with a suitable factor for the spoiling effect.

Notation

Ω Angular velocity in radians per second.
V Velocity of advance, Fig. 7*a*.
u Axial velocity through airscrew, Fig. 7.
u_1 Axial velocity in wake, Fig. 7*a*.
W Resultant velocity relative to blade element, Fig. 7*b*.
α Incidence.
θ Blade angle.
ϕ $=\theta-\alpha$.
ω Angular velocity just behind airscrew, Fig. 7*b*.
a_2 $=\frac{1}{2}\omega/\Omega$.
R Section 4, airscrew radius. Section 10, body drag.
r Radius of element.
b Section 4, number of blades. Section 10, coefficient in equation for drag.
c Chord.
T Thrust.
Q Torque.
k_L Lift coefficient of blade element, infinite aspect ratio.
k_D Drag coefficient of blade element, infinite aspect ratio.
P Resultant pressure on nose, Fig. 9.
h $dP=hdT$. Section 4, equation (3).
F $=1/4\pi\rho ru^2 \cdot dT/dr$. Equation (7).
Dashes distinguish quantities altered by the presence of the body, Fig. 9.
A, B and C Components of loss of energy, Section 5.
R_v, R_t Components of body drag, Section 10.
$T_2=T-P$ "Effective thrust."
$T'=T-R$ Net thrust.
T_c, R_c Coefficients of the type $T/\rho V^2D^2$.

List of References

(1) R. & M. 786. An Aerodynamic Theory of the Airscrew. Glauert.
(2) R. & M. 830. Experiments with a Family of Airscrews. Part II. Fage, Lock, Bateman and Williams.
(3) R. & M. 892. Analysis of the Family of Airscrews by means of the Vortex Theory and Measurements of Total Head. Lock and Bateman.

(4) R. & M. 953. Experiments to Verify the Independence of the Elements of an Airscrew Blade. Lock and Bateman.
(5) R. & M. 955. The Measurement of Airflow Round an Airscrew. Lock and Bateman.
(6) R. & M. 956. The Airflow Round a Body as Affecting Airscrew Performance. Lock, Bateman and Townend.
(7) R. & M. 1030. The Effect of Placing an Airscrew in Various Positions within the Nose of a Streamline Body. Bateman and Townend.
(8) R. & M. 1046. The Effects of Body Interference on Airscrew Performance. Jennings.
(9) R. & M. 1120. Analysis of Experiments on an Airscrew in Various Positions within the Nose of a Tractor Body. Lock.

Discussion

The President: The Society was very fortunate in having Mr. Bateman to take Mr. Lock's place at such short notice, though he supposed the task was not specially difficult because Mr. Bateman had, with Mr. Lock, been responsible for this involved research. He called upon Mr. Fage, who had also been closely concerned with this work, to open the discussion.

Mr. Fage: The paper was a decided advance towards the solution of the airscrew body interference problem. It seemed that the simplest part of the problem was the interference of the body on the airscrew; the other and more difficult part was the interference of the airscrew on the body. Mr. Lock had divided this latter interference into three parts: (1) A pressure effect; (2) a velocity effect; and (3) a spoiling or turbulence effect. Of these three parts, the first appears to be adequately dealt with by Mr. Lock's theory, the second can be estimated from empirical relations based on experiment, and the third is uncertain, but may be important. This statement of the three parts of the interference, makes one realise the inherent difficulties in the problem and helps one to assess the good work which Mr. Lock had done.

There was another point which could not fail to impress the reader of the paper, and that was that Mr. Lock and his colleagues had developed the technique of airscrew model experiments to a fine art. He hoped that Mr. Lock would continue his experiments on pusher bodies, and with the present paper before them, the members of the Society would eagerly await the results of these latter experiments.

Dr. Douglas: In these experiments rather large bodies had been used in relation to the airscrew diameter in order to magnify the effect. He was not entirely convinced by Fig. 23 that they did represent reasonable proportions, because the views of the aeroplane bodies were side elevations, whereas the model was a solid of revolution. He thought the principle of using a model which does not represent actual proportions was one which must be used with great caution, but it is legitimate as a means of checking a theory.

There was one other point. He thought Mr. Bateman had made it clear that these excrescences added to the body had about doubled the drag one would estimate from the increased velocity in the slipstream. This cannot be a general conclusion and he wished to have Mr. Lock's opinion on the matter.

Mr. Ralli: The work of Mr. Lock and his collaborators had shed considerable light on a very intricate problem. Even if the members did not carry away from the lecture a ready made solution for every conceivable case, it was a great advantage to have as much as a qualitative conception of the phenomena involved. The speaker doubted whether it could be more than qualitative. The various sources of interference had now been classified. They could be divided into (1) a pressure distribution on the nose; (2) a V^2 effect on the body as a whole,

and (3) a spoiling effect. The V^2 effect had been in regular use for many years by all designers, and the spoiling effect was also strongly suspected and allowed for empirically.

The pressure distribution on the nose, however, was a new feature. The essential result was that there was no loss of energy from that source for a given Kq as long as the streamlines were not broken up or unduly deflected. The experiments confirming this statement were all based on bodies of revolution, and the calculations borrowed from pure hydrodynamics also applied exclusively to the ellipsoids of revolution.

There were very few water-cooled engine mountings which were bodies of revolution, and with regard to air-cooled engines the cylinders were so close to the propeller that it appeared questionable whether the argument concerning the continuity of the streamline was valid on any type of actual machine. Alternatively, if the theory were valid h would be difficult to estimate. Where the results found on bodies of revolution were not applicable one was obviously thrown back on the existing practice of introducing a V^2 effect and magnifying it empirically. The correction to torque would not then hold good either. That the necessity for some such correction existed would probably have been found by most designers. A striking case occurred recently at the Fairey Aviation Company, when a light plane propeller fitted in succession to two different makes of aircraft with identical engines gave a difference of 170 r.p.m. at approximately the same level flying speeds. As the bodies were not available for analysis, a study of h was not possible. Roughly the maximum cross sections were very different. The type of engine was with air-cooled cylinders in line very close to the propeller. A good blunt-nosed spinner was fitted. The theory might, therefore, be expected to be in error, and it did appear at first sight that 170 r.p.m. was a considerable difference which would imply a larger value of h than could be found on an ellipsoid of revolution, in effect 27 per cent. on Kq. The difference seemed rather large, but he had no doubt that experiments would be undertaken to resolve this problem as well as others. Further model tests with actual typical engines were desirable. He wished to thank Mr. Lock and his collaborators for this contribution, which would prove of great value.

Mr. BRAMSON: He desired to level a criticism, not so much at this particular paper as at this type of paper and its method of presentation (not, of course, the delivery of it by Mr. Bateman, which had been excellent). What he meant was this: Either the members had time and opportunity to study the mathematics of such a paper before coming to the lecture or they had not. If they had such time and opportunity, and took advantage of it, then there did not appear to be any particular need for the author in presenting the paper to go through the mathematical part of it. If, on the other hand, there had not been time and opportunity to assimilate the mathematics before arriving at the meeting, he for one found it completely impossible to follow them while the paper was being delivered. That might be entirely his own fault, but he had reason to believe that other members shared his deficiency. Would it not be preferable, as a matter of lecture technique, when the subject matter of a paper did involve complicated mathematics which are available in print beforehand, for the lecturer to give merely a verbal summary stating the nature of the investigation made and the results?

At the end of the paper appeared a list of symbols, and in no case except one—that of angular velocity, which was expressed in radians per second—were the units given, nor was there any indication of the system of units; yet this was surely necessary in order to give any significance to the constants. The whole paper seemed to be rather concerned with the establishment of certain somewhat difficult constants. In the list of notations he had tried to discover what h really was, for this seemed to be an important quantity, and he did not

know what it represented. But in the list of notations he found $hdP=hdT$, and there was a reference to Section 4, equation (3). On looking this up he found that the quantity h was not in that equation. He imagined that this was a printer's error, but it was rather unfortunate because it concerned such an important quantity.

This, of course, must not be taken as a criticism of the paper, but rather as a suggestion for an improved technique that would render these lectures as intelligible as possible.

There was one point about the subject matter to which he desired to refer. With very few exceptions, bodies could not be considered as being detached from wings. To what extent, then, was it really profitable to speculate on and investigate the mutual interference between an airscrew and a body apart from wings? It was known that the effect of the presence of wings either at the bottom of the fuselage, at the top, or in the middle, must be very considerable indeed, and there must be interference both between the airscrew and the wings and between the body and the wings. He would like to know the lecturer's opinion as to the nature of that interference. He was referring to the effect of a spiral flow round the body, striking the wing roots downwards on one side and upwards on the other. He desired to thank the lecturer for his most able paper.

Mr. J. D. Blyth: It is difficult to see precisely how the methods described in this lecture are to be applied in performance calculations, as these are generally made some time before the airscrew is designed.

It is shown in Fig. 19 that immediately behind the airscrew there is a large increase of pressure, giving rise to pressure drag. It has also been shown from time to time that, given a suitable shape of nose, the efficiency of the airscrew is increased, and also that maximum efficiency occurs at a greater value of V/nD.

In R. & M. 458, the conclusions arrived at after tests on certain airscrews were that at the maximum speed of the machine the thrust and efficiency were increased by 3 per cent. and 6 per cent. respectively, while the rotational speed was decreased by 3 per cent. The value of 6 per cent. for the increase in efficiency seems perhaps a little high, but the general result is what would be expected if the experimental mean pitch were increased.

It is not clear whether the increase in pressure shown in Fig. 19 is of the same order at all values of V; it seems possible that the effect would vary with forward speed, and would be greater at low values of V than at high values, since at low values of V the value of Vs/V is greater (Vs being the speed of the slipstream). This is to some degree indicated in Fig. 20, as the pressure drag increases with Tc.

He would suggest that the increment in resistance, due to pressure drag, does to a great extent balance out with the increased airscrew efficiency; leaving two factors to be taken into account, first, the increase in drag of the parts in the slipstream on account of the increased velocity of the slipstream; and secondly, the variation in torque horse-power absorbed by the airscrew, due to body interference from that which would be absorbed by the airscrew rotating in free air. This may be termed the variation in "effective" experimental mean pitch from "designed" experimental mean pitch, and the difference would apparently vary with Vs/V.

If it were possible to plot curves giving the relationship between "P_E effective" and "P_E design" (P_E being the experimental mean pitch) at maximum or design speed for various values of airscrew diam./body diam., and also of the same relationship for a fixed value of diameter ratios and varying values of V/V max., he felt that a very considerable part of the difficulties encountered in performance calculation would disappear.

Mr. Lobelle: He suggested that the experiments might be repeated with different engine arrangements, using the apparatus described in the lecture to differentiate between airscrew thrust and the various sources of body drag.

There need not be a very large number of engine types, the principal ones could be: Radial with closely packed cylinders, radial with widely spaced cylinders, and water-cooled completely faired in, but exhibiting the oval cross section found in practice.

Such experiments should be made on models which resemble as closely as possible an actual typical machine. He thought the influence of the shape of the spinner was not very great, but the unavoidable obstructions behind the spinner were very important.

It would also be interesting to investigate by these methods the interference effect of propellers placed in close proximity to thick wings.

Mr. SCOTT HALL: There was one small point he desired to raise on the paper. In most modern radial air-cooled engines the exhaust issued almost straight from the cylinders at right angles to the slipstream. Instead of the kind of representation which had been shown on the model, with merely the cylinder heads projecting round the body, one had in effect almost a flat ring all round the body, and it seemed to him that the interference effect of these exhaust gases issuing at high velocity—as they must with the engine running full out—was likely to be considerable. The same thing was present to a less extent on water-cooled engines in high performance machines. He thought that something might be gained by deflecting these gases along the line of the slipstream. He had only seen this done once without excessively long uncowled stub pipes and that was on a French machine, the Nieuport Jockey.

Mr. OWNER: The paper seemed to be a very laudable effort to reduce this interference to some rules and regulations without being entirely empirical in the process. But there were one or two matters to be considered from the point of view of guiding the unfortunate person who had actually to deal with these interference effects. He noticed in Fig. 19 that a pressure grading was given along the axis of the body. This showed, as was expected, a very high pressure immediately behind the airscrew at large thrust conditions. It seemed that this should affect the increase of drag caused by excrescences, such as undercarriage or engine cylinders, or similar bodies placed in the slipstream. It was therefore a matter of some importance, and one which he thought would repay investigation, to determine the relative axial position of the excrescences which had been tested. In this connection Fig. 21 was interesting. It referred to a plane of 1.0 diameters behind the airscrew in the forward position. He wished to know whether that diameter referred to the maximum body diameter or to the airscrew diameter. If it referred to the airscrew, as he imagined, the excrescences were rather further behind than the position in which one usually found them. This amplified the remarks which Mr. Bramson had just made. In practice, the drag was likely to be more serious than the effect upon the airscrew's performance. Consequently, if the experiments on the excrescences could be extended and amplified with positions much nearer the screw, it would bring this work more within the range of practice as one found it.

A further point arose in this connection. If one could make the excrescences far enough back in the low pressure or negative pressure region, it appeared that they should not affect the airscrew or body drag very materially. If this was the case, it would be a point of some value. Again, Figs. 4 and 5 indicated what a large effect the projections might have upon the course of the airstream. One usually assumed that the streamlines close to the body were more or less parallel to the tangent of the particular point on the body with which one was dealing. But it appeared that the air interference might alter the direction of this flow. He believed that Mr. Bradshaw did some work a few years ago on the relative cooling efficiency when an airstream was directed at varying angles. Further work on these lines, taking into account the various positions of the excrescences, would be very helpful.

In connection with the spinner, according to the paper the effect of the spinner was fairly considerable in reducing drag. Recently there were some spinner tests published in the N.A.C.A. Report, No. 301, in which report it was stated that the effect of the spinner on the total drag was of the order of 1 per cent. Could the lecturer give any information as to whether the spinner really did affect the total drag when one came to the order of interference which was likely to be met with in a full-sized plane, or whether the American view was the correct one? No one liked spinners if it were possible to do without them, and if the order of effect was as small as the American experiments would indicate, one would prefer to do without them.

The PRESIDENT (Colonel the Master of Sempill) expressed the thanks of the meeting to Mr. Bateman for the excellent way in which the paper had been presented by him, and asked him also to tell his colleague, Mr. Lock, how much his paper had been appreciated. At the same time he hoped he would draw Mr. Lock's attention to the remarks of Mr. Bramson and others with a view to amplifying the paper so as to deal with certain points in a broader manner.

Mr. WHYBILL E. PARK (*communicated*): The paper is a very valuable contribution to the theory of airscrew design, presenting a very logical summary of the results of wind tunnel experiment. It does, however, emphasise the necessity for further evidence, both wind tunnel and full-scale, before the theory can be used with complete confidence for the design of aeroplane propellers.

The assumption that the parameter "h" is independent of the type of airscrew seems at variance with the earlier assumptions and with the results obtained in practice, and he would imagine that the form of the root sections could have a measurable effect on the interference at the body.

With the normal type of wooden airscrew the root sections are generally fixed by strength considerations, without much possibility of variation, but the trend of development, with the use of high speed radial engines involving small diameter propellers and the spinner boss, together with new constructions permitting smaller root sections, indicates their importance in the matter of body interference.

In the design of propellers from the information usually available the calculation of "h" from comparison with wind tunnel evidence of a family of body forms, will present some difficulty, and the full value of Mr. Lock's paper will be realised when the analyses of many types of existing propellers gives more reliable comparison.

He would be interested to know if it is possible to calculate from results obtained with one propeller on a particular machine, the probable effect of slight variation in *type* for the same machine, without *exact* knowledge of the body form, also if there is any evidence available as to the effect of body interference on airscrew vibration.

Dr. E. G. RICHARDSON (*contributed*): Mr. Lock has the merit of working out a theory for the design of an airscrew in airflow of so complex a nature that it would seem impossible to specify the physical conditions, and of showing by experiment that this theory is not merely academic, but succeeds in practical applications remarkably well. Troller has recently published in the Z.F.M. (page 325, 1928) another method for airscrew design when body interference has to be taken into account, but does not support his theory by *ad hoc* experiments. He had not had the leisure to examine Troller's method closely, but apparently he works out the theoretical flow past the body by imagining a series of sources and sinks at given points, and then applies the Drziewcki theory to integrate the total thrust on the blades. He gives curves of thrust grading in the style of Mr. Lock's Fig. 15. He suggested it would add to the value of Mr. Lock's paper if he could give a brief comparison between his own and Troller's method.

He noted that the spinner does not materially interfere with the performance of the screw. Has the lecturer any comparative data on the flow with and without spinner in the absence of the "efficient" part of the blades?

Reply to Discussion

Mr. Lock (*communicated*): He agreed with Mr. Fage that the turbulence or spoiling effect was a point of great future practical importance and that the experiments described here, though sufficiently accurate to establish its existence, threw little light on its cause.

Dr. Douglas remarked that Fig. 23 is misleading as it compares the larger dimension of the cross section of an actual body with the cross section of a body of revolution. This is admitted except that the effect on the airscrew depends chiefly on the portion of the body immediately behind it which is likely to approach more nearly a shape of revolution than the remainder of the body.

With regard to Dr. Douglas' second point, it is mentioned in the printed text of the lecture that the factor b for the drag of excrescences was slightly less than 4 as compared with 5.0 for the bare body and 2.5 for the theoretical increase of square slipstream velocity. Thus there appeared to be still a considerable percentage turbulence drag with the excrescences though less in proportion than for the bare body. He agreed that these figures could by no means be taken as universal on the limited evidence and that for some shapes the turbulence drag might be much smaller. The relative values of velocity and turbulence drag deduced for the bare body were borne out by the pressure plotting experiments, but these were not available for the body with excrescences.

In reply to Mr. Ralli, he admitted that there was still a wide gap between the solution of a theoretical problem and the application of these results to all practical cases. But admitting that the conclusion of no loss of energy due to the pressure on the nose would not hold when there was an air-cooled engine immediately behind the screw, it should still be possible to obtain empirical values of h by the method of total head observation; the idea of separation of the drag into three parts would still be useful and the only difference would be that a part of the form drag on the cylinders would have to be included with the V^2 and spoiling effect. This case had been tentatively included in a published report, Reference (9), but was omitted in the lecture partly for simplicity and partly owing to the absence of experimental data. Again, if the body was not one of revolution it should not be difficult to guess a shape of revolution which would have a similar effect on the screw. The case mentioned of a large difference of revolutions with the same airscrew and engine but different bodies would be a very interesting one for further study. A change of k_Q of 15 to 20 per cent. was observed as an effect of the bluff-nosed body B of Fig. 1, but this was doubtless of larger diameter than the case mentioned. The presence of cylinder heads would however tend to increase the effect. The second body might have been such as to give a negative effect which had been shown to be possible. In any case he welcomed the information as showing that the problem was of importance.

In reply to Mr. Bramson he admitted that he had made the attempt and failed to convert the subject matter of the paper into non-mathematical form for the purpose of oral presentation. It would certainly have been possible to describe experimental results and conclusions in non-mathematical form, but in a case when the essential point of the paper was contained in a few simple mathematical equations it seemed better to attempt to explain them. This he believes it was possible to do and he regretted if Mr. Bateman and he had failed in the attempt.

As regards notation and systems of units he might point out that all numerical results given in the figures or elsewhere were expressed in non-dimensional coefficients all of which were self-explanatory, with the exception of the ordinary thrust and torque coefficients k_T and k_Q and the thrust grading which is defined so that the area under the curve is equal to k_T. The definitions of k_T and k_Q are now printed inside the cover of every copy of the R. & M. series and were considered to be sufficiently well known.

He apologised very humbly for the existence of two misprints in the one definition of the important quantity h. This quantity was non-dimensional as appeared from the (correct) definition; the true definition was also included in the sheet of formulæ which Mr. Bateman suspended in view of the audience for the whole lecture.

The suggestion about wing interference was very interesting and one agreed that this was one of the most important lines of research which still remained to be undertaken. One felt that in most cases the direct effect of wings on the airscrew was of less importance than that of the part of the body close to the screw, but it was certainly not negligible. One was prepared to support the policy of arranging experiments in order of simplicity, taking the combination of body and airscrew first, then wings and body and finally all three together.

The direct effect on lift of the spiral outflow striking the wings should be small as the upward flow on one side should compensate the downward flow on the other side. The chief effect would be to assist in producing the rolling moment on the aeroplane which partly balances the reaction of the torque on the airscrew.

The direct effect of the slipstream velocity on the wing must be of importance, and little experimental evidence was available at present.

Mr. Blyth's request for some empirical relationship to take the place of the detailed analysis given here was most reasonable, but the difficulty in practice was that the effect on the airscrew varied so critically with its exact position in the body. Any empirical rule connecting the change of torque with the relative size of body and airscrew for a normal type of machine might be expected to have such large and numerous exceptions as to be of little value. This point could only be settled by many laborious experiments on models of actual machines.

Mr. Lobelle also makes suggestions for further experiments which together with the suggestions made by other speakers were of great value and interest. While agreeing as to the desirability of experimenting with models of actual machines and engines, one was prepared to defend the policy of the A.R.C. of putting experiments on ideal shapes first. It could certainly be said that no theory of the kind here described could have been developed on the basis of experiments with actual machines only, so that if the theory proved of any value, that would help to justify the experiments.

One had no data on which to reply to Mr. Scott Hall's suggestion of the importance of the velocity of the exhaust gases. He believed the effect to be small, but would certainly think that there might be some advantage in discharging the gases backwards instead of sideways.

In reply to Mr. Owner, the important factor influencing the drag of excrescences would be the velocity over them rather than the local pressure. The velocity did not vary very greatly for different positions along the body. The distance of the excrescences behind the airscrew of one airscrew diameter was expressly chosen for the sake of simplicity as the least distance at which the torque was unaffected. It would certainly be interesting to experiment also with excrescences so close as to affect the torque of the airscrew, but one imagined that this should not affect their drag very greatly for the reason already mentioned. The same argument applied to the suggestion of putting the excrescences further back still.

He admitted that there was no direct evidence of the effect of a spinner on the overall drag. One might expect that if the body was otherwise very well streamlined, then it would be important to fit a spinner and its absence would increase the drag, but that if the body were unavoidably of a bad shape to start with then a spinner might make little difference.

Written Communications.

Mr. Park.—In stating that the value of h was independent of the design of airscrew one was thinking chiefly of such variation as change of blade width, number of blades, and moderate changes of pitch. He admitted that large changes of the relative thickness of root and tip sections might modify the results appreciably, but the fact that the value of h could be calculated for an ellipsoid of revolution from considerations which had nothing to do with the particular design of screw, pointed against this.

In calculating the effect of slight changes of propeller design with a given machine, the effect of body interference might reasonably be neglected altogether.

In reply to Dr. Richardson, he had previously glanced at the paper by Troller, and had been interested in the method of approximating to the shape of a body by means of sources and sinks (which was not peculiar to that paper), but he considered that the use of an ellipsoid was less trouble. As regards Troller's application of airscrew theory he had not, in his ignorance of German, studied it sufficiently closely to understand it completely, but he did not think that a method could be successful which did not depend on numerical data for a particular airscrew.

As already mentioned, he had not actually experimented without a spinner.

In conclusion, he wished to thank all the speakers for their many helpful suggestions for future experiments. He had felt from the first that he had a subject for his lecture on which he might hope to gain much useful information from his audience and he had not been disappointed.

PROCEEDINGS

FIFTH MEETING, SECOND HALF, 64TH SESSION

In the Chair:

THE PRESIDENT, COLONEL THE MASTER OF SEMPILL AND MAJOR J. P. BULMAN

A meeting of the Royal Aeronautical Society was held in the Lecture Theatre of the Royal Society of Arts, 18, John Street, Adelphi, London, W.C.2, on Thursday, March 14th, 1929.

The PRESIDENT: He thought the first thing they would like to do would be to approve a telegram of congratulation which had been sent to Captain Irving, at Daytona. Captain Irving is an Associate Fellow of the Society. He was sure they would wish to congratulate him, and also Captain Wilkinson, on the excellent work that they had done in connection with the production of the car which had recently captured the world's speed record. The lecturer that evening, Wing Commander Hynes, was one of the true pioneers who had had a very lengthy experience of aeronautics from almost every conceivable angle. It was very largely due to him, and also to Major Bulman, who had kindly consented to take the chair later on, that British aircraft engines had reached the very high level so universally recognised in other parts of the world. Wing Commander Hynes, in 1908, when he was a Gunner, obtained permission to attend a course at the old R.E. Balloon School, at Bournville, and he had had a good deal of experience then with captive and free balloons and Cody man-lifting kites. After that he went abroad. He returned in 1910 and joined the old Blériot School at Hendon and took his ticket, which was No. 46. In 1911 he joined the Air Battalion under Captain Fulton, and was subsequently transferred to the Royal Flying Corps on the formation of that body. Since then he had occupied many very important positions, which had well qualified him to give a lecture on the subject which was before them that evening. With their permission, he would ask Major Bulman to occupy the chair, and would call upon Wing Commander Hynes to deliver his lecture.

ENGINE PERFORMANCE TESTING

BY

WING-COMMANDER G. B. HYNES, D.S.O., R.A.F.

The title of my lecture is one which might well be regarded as covering a very wide field, and in deciding what aspects I should attempt to cover I have found some difficulty in selecting my material.

A good deal of the work is rather prosaic and dull, and quite familiar to many of you, and it would seem that no good purpose will be served by devoting our time to considering testing in too much detail. I propose therefore to deal with the subject in a fairly general way, and I hope that on my part by keeping my lecture reasonably short there will be opportunity to bring out points of interest that may have been omitted in the subsequent discussion.

As is customary and necessary, I must remind you that the Air Ministry accepts no responsibility for any of the remarks I may make or the conclusions drawn, which are purely my own personal views.

Testing for performance may, I think, conveniently be divided into two clear-cut categories, viz., Ground Testing, and Tests in the Air.

Ground Testing

In the matter of plant and apparatus for measuring performance the facilities available are fairly complete, and the technique of testing has been reduced to something approaching an ordered process, but it must be borne in mind that the data so obtained are primarily a measure of the performance and efficiency of the engine as a prime mover, and a large gap is left in the knowledge which is really the essential point, viz., the merit of a given power incorporating such prime movers as a propelling plant for aircraft.

Plant

Considering briefly the plant available for ground testing, we have the following apparatus and facilities:—

For power measurement, in England it has always been held that for accurate and reliable observations, and ease of manipulation, a method of power absorption must fulfil the requirements that the brake torque must be capable of variation and adjustment without stopping the engine, and the b.h.p. developed is to be measured by means of the torque reaction of the brake or engine.

There are two well-known forms of brake which conform to such requirements, and they are the Heenan-Froude and Heenan-Fell types.

I do not propose to describe them in detail as this would take up too much time. It is sufficient to say that the former is a hydraulic means of absorbing the power generated, and the power output is computed from the torque reaction of the brake.

In the Heenan-Fell type, power is absorbed by turning what is colloquially known as a "wind stick"—that is a two (or more) armed fan with plates on the tips of the arms in a specially constructed casing, which provides a means of varying the power absorbed by the "wind stick," and measuring the power developed by the engine from the torque reaction of the engine cradle.

Each of these brakes has its points to recommend it, but these I cannot discuss in this paper, except perhaps that I should mention the feature of the Fell type, whereby the air disturbance created by the fan can be used to produce the cooling draught necessary to cool air-cooled engines.

Other means of power absorption such as calibrated airscrews, simple fan dynamometers, etc., are ruled out for performance testing in which anything like accurate observations are necessary, owing to the large errors to which they are susceptible from a variety of causes, and the uncertainty of the different corrections which may have to be applied. To cite only two instances, wind and interference effects alone may cause variations as great as 20 per cent., and in extreme cases I believe even more.

For cooling the engine in the case of water-cooled engines simple means suffice, and the problem of regulating the cooling to any required degree is easily solved by control of the water supply. In the case of air-cooled engines, a cooling blast is obtained either from a separately driven fan supplying air to a duct in which the engine is placed, or by means of the Heenan-Fell brake.

In the past it has been the practice to impose only simple limitations of maximum permissible wind speed, but experience has shown that this delightfully simple scheme may give rise to misleading optimism as to cooling characteristics, when the engine comes to be installed in an aircraft.

It is essential that engines should not be overcooled on the test bed, and it is realised now that the real criterion is cylinder temperatures which must be comparative to the cylinder temperatures experienced under the most adverse conditions of flight.

Wherever possible, therefore, prior to an important test such as the type test, the temperatures of an experimental engine will be ascertained by flight tests at full throttle in a suitable aircraft, and then an appropriate correction added to cover tropical conditions, and also a margin allowed for normal conditions.

Where such flight test figures are not available, an arbitrary figure of 150°C. above the cooling air temperature may be accepted as a temporary measure, until the temperatures determined in the proper manner are forthcoming.

The temperature measurements are quite easily and conveniently made by means of thermo couples, of which the sparking plug washer pattern is the simplest and best known type.

The regulation of oil temperatures on the ground presents no serious difficulty.

For exploring the internal pressure conditions and certain other phenomena of combustion and such like, indicators are available which may be of the optical or electrical types, of which latter the Farnborough electrical indicator is an excellent example.

In research and early experimental work this aid can be of the utmost value if fully made use of, and the significance of the "writing on the wall," in other words the diagrams and the tale they can tell, are correctly deciphered and interpreted.

In fact, I am not going too far in saying that it is within my personal knowledge that the performances achieved in certain experiments were only made possible by its use.

For fuel consumption measurements there are available reasonably accurate flowmeters, or calibrated tanks can be used, and specific fuel consumptions can consequently be quickly and easily determined.

Temperature and pressure measurements for various purposes can, broadly speaking, be made with ease and accuracy, and full use can be made of the very valuable aid to be gained from the senses of sight and hearing, which in the case of skilled operators become remarkably acute.

For critical investigation of more elusive problems, for instance torsional vibration, which has been the cause of a number of failures in the past which were not then fully understood, but of which the workings are now being elucidated, there is the torsio graph, and then again for the study of moving parts the oscilloscope can be brought into action.

I have laid some stress on this side of performance testing on the ground, as the contrast is rather striking when one comes to consider the question of testing in the air and the difficulties associated with it.

Testing

Turning now to actual testing.

Initial Experimental Running

In the design of a new engine there is the preliminary stage of experimental running which follows no set course, but may be said to comprise proving the mechanical design, and making a more or less great number of changes, alterations and improvements varying with the intrinsic merit of the original basic design, coupled with a strong dash of that elusive quality, good luck or blind chance, however one cares to put it. These experiments range over the whole gamut of such matters as mechanical strength of parts, valving, valve timing, compression ratios, induction systems, carburation, cooling, and a host of details which go to make up the success or otherwise of a new design.

The apparatus which I have already described is sufficiently comprehensive to enable those in charge to determine easily and accurately the effect of the various changes on such vital points as mean effective pressures, specific fuel

consumptions, etc., and consequently progress is mainly dependent on the ability of the operators to profit by and make the best use of all the information which is at their disposal.

Type Test

After emerging from the initial experimental stage, an engine then reaches the point at which it is subjected to the first stabilised form of performance testing—that is to say, the type test.

This test has been evolved as the result of long experience, and is intended to establish the main characteristics of the type, and to prove that it has reached a certain standard of mechanical reliability, and the completion of a successful type test is the point at which in official eyes an engine becomes " airworthy." It is worthy of note in passing to mention, however, that such is the difference in operation on test beds on the ground, and in an aircraft in the air, that the engine which has gained its title to " airworthy " is seldom a thoroughly reliable, trustworthy and efficient engine in the air until after a considerable length of " air apprenticeship " has been served, as the tale of subsequent modifications will show. There are, of course, exceptions, but mainly in the smaller and simpler types. With the actual tiresome details of the test I do not propose to bother you, but I might refer to a few of the salient features.

Power curves are taken at the beginning and end of the test to establish this characteristic, and to check the rating and depreciation. Mechanical reliability is proved by endurance tests of 100 hours for Service and 50 hours for civil engines, followed by examination of the parts after strip. Fuel and oil consumptions are measured and checked against declared figures. A margin of safety for over-speeding and high power are catered for by the high speed and high power tests.

Type Test for Supercharged Engines

For normal engines the conduct of the type test is quite straightforward, but the appearance of supercharged engines designed for the maintenance of constant power to various altitudes has introduced a new problem; in the case of the supercharged engines of the ground boosted type the problem does not arise unless ground boosting is associated with a degree of maintenance of ground power, which as a matter of fact it may well be. Here we are faced with the proposition of an engine designed for maximum performance at altitude and which has to be tested under ground conditions.

To provide a plant which will even approximately reproduce altitude conditions of temperature and pressure at ground level is too difficult technically, too extravagant in operation and quite impracticable financially, to be considered as a normal piece of plant for engine constructors. A scheme has been evolved and issued as an addendum to the official schedule of tests, and is now on trial at works where supercharged engines are being built.

Briefly summarised, it is a scheme whereby the pressure at the air intake is artificially restricted to a predetermined figure, and empirical corrections are applied for temperature conditions and variation in exhaust pressures, etc.

Addendum " A " is a rather formidable looking document introducing a number of new terms, such as " Rated Boost," " Rated Altitude," " Rated Full Power," and so on, and is liberally fitted with cabalistic signs, formulæ and tables, and is calculated to give the ordinary tester a bit of a fright at first sight, but it is not so bad as it looks, and the tables have been got out to give relatively simple omnibus corrections.

In view of the uncertainty which must always accompany large corrections, the absolute value of a test under such conditions must be open to a considerable measure of doubt, but as a practical solution of a difficult conundrum, I think the proposal, in the absence of anything better, is on right lines, and will be found

eventually to give comparative results of the right order, at any rate for engines with supercharger units designed to work up to moderate altitudes of, say, 10-12,000 feet. It is recognised that it is the temperature correction which is most troublesome, both on account of the general uncertainty of the exact law of variation of power with temperature over wide ranges, and also because it may be stated with confidence that the usually accepted formula of variation of indicated horse-power with the square root of the absolute temperature undoubtedly breaks down, when the temperatures are high enough to be associated with the onset of detonation.

I may add also that the difficulties of evolving even an empirical solution are greatly intensified by the very great lack of any reliable data for performance figures under the actual design conditions. The reason for this will be shown in the latter part of my paper.

Tests for High Compression Engines

The tests for high compression engines are set out in Addendum B. of A.P. 840. The type of high compression engine referred to is, of course, that in which the compression ratio is higher than that capable of being used on the ground without detonation on the fuel normally to be used in service, and in which the throttle is not fully opened below some pre-determined altitude. The most important part of this Addendum describes alternative methods of rating the engines. At the option of the constructor, such engines may be rated in accordance with the procedure for a supercharged engine, or doped fuel may be used so that full throttle readings can be obtained on the ground. When this latter artifice is employed, then the b.h.p. corrected for pressure and temperature at ground level is corrected to the rated altitude, by means of a table which is given, and the resultant figures are the rated horse-power. Testing is then carried out generally on the lines of the normal A.P. 840 procedure, the endurance tests being run at 90 per cent. of the rated power plus an additional allowance of 1 per cent. for each thousand feet of rated altitude on account of the greater tendency of a high compression engine to detonate when the charge density is reduced by throttling in place of reduction due to reduced atmospheric density.

It would have been possible, of course, to discourse at some length and in greater detail on this type testing, but it would have necessitated adding A.P. 840 and Addenda A. and B. as Appendices to the paper. To do this would have involved a large amount of printing, and in the case of Addendum A., troublesome printing, and would, I think, have hardly been in keeping with the general character of the rest of the paper. I have confined my remarks therefore to general references only, but this, naturally, does not preclude questions of general interest being raised in the discussion afterwards.

Production Test

It may be stretching a point to regard another ground test, viz., the production test for series engines, as performance testing, but I think it is legitimate to regard it as such. It is a simple routine test with no frills certainly, but as the result of experience has been shown to be adequate to demonstrate that the series produced engines are up to the standard of performance of the type engine in all important essentials.

Test after Overhaul or Rectification

Similar remarks to those in preceding paragraph apply.

With this I propose to leave the subject of testing for performance on the ground, as I hope that the general outline which I have given demonstrates that this branch of the art is established on a reasonably firm basis.

Air Performance Testing

Consideration of performance testing in the air reveals the very wide gulf which exists between it, and the ground testing which I have just dealt with.

Mechanical aids are much more limited or entirely lacking, the engine may be regarded in all conventional types of aircraft as inaccessible, and the making and recording of observations very much more difficult, and the whole fact remains that no really practical piece of equipment has yet been evolved. From the mere fact of flying, in fact when one considers that a pilot and an observer, or in many cases pilots alone, have to do this work strapped in their seats, lumbered up with flying clothing, hampered by flying helmets, goggles, probably oxygen masks and inter-communication telephones, and wearing thick flying gloves and parachutes, it is a very remarkable tribute to their skill and determination that such, on the whole, reliable and accurate results can be obtained as are actually achieved. To do this class of work under such conditions, it will be appreciated that pilots and observers too must have the triple gifts of aptitude, special training and experience.

Apparatus

If following similar lines to the first part of the paper, one surveys the mechanical apparatus available for measuring performance in the air, the first and perhaps most striking deficiency is the want of any simple and direct means of determining the power output of the engine when in flight.

From this cause alone a crop of difficulties are immediately apparent, as it is hardly necessary to stress how closely various aspects of engine performance are linked up with the power an engine may be developing under any given set of conditions.

The most ambitious attempt to meet this want, with which I am acquainted, was the apparatus first designed, I believe, in Germany and known as the Bendmann hydraulic dynamometer hub. The hub was schemed out in such a manner that records of the engine torque and thrust were obtained by means of small hydraulic rams operated by oil pressure.

The apparatus, however, had a number of defects and inherent possible sources of error, is troublesome to calibrate and is most clumsy to instal, and has not, so far as I am aware, been made use of in any general way. Barring one American N.A.C.A. Report on some tests of this type of hub, I have seen no accounts of any serious employment of it in the air. Certain alternative proposals for other forms of dynamometer hubs have been suggested, but the fact remains that no really practical piece of equipment has yet been evolved.

Perforce, therefore, estimations of engine power have to be made by less direct means, which are in consequence more laborious and less reliable. Probably the best evolved so far is that which was suggested in the first instance, I think, by Mr. Capon. At any rate, so far as I am concerned, it was he who first advocated its use. In this scheme, by making certain assumptions and determining the K_t and K_q characteristics of an airscrew, one can from aerodynamic considerations use the airscrew as a dynamometer. Even so, however, it is a very different matter to being in a position to get a flexible, direct and accurate reading.

Normally, however, one has to fall back on various devious methods of computation which are all open to suspicion.

To support my point, may I quote one excellent example which was within my own experience. In the course of certain experiments on an aircraft, the results which we were getting were much below expectations, and endless argument ensued as to whether the aircraft and/or airscrew were at fault, or the engine was not giving the power it was supposed to be.

The engine power was computed from records of r.p.m., fuel consumption, boost pressures and temperatures, back pressure, etc., and duly corrected from various test bed and other data; on the other hand, from aerodynamic considerations the power required for the performance was something quite different, the discrepancy being very large indeed.

According as one might be an engine or aerodynamic protagonist, a perfectly sound and apparently logical argument was possible to show that the other side must be in the wrong!

When I terminated my connection with the work the divergence of views had not been resolved, and for all I know may never have been satisfactorily explained.

Probably neither side was absolutely correct, but there was no certain means of settling the extent of the errors either way. A single reliable power measurement would have dispelled at once the fog of uncertainty.

In the past, measurement of fuel consumption was anything but easy or accurate, and it has often been necessary to adopt very cumbersome and laborious expedients.

The want of a suitable flowmeter has been fully appreciated for years, and many and varied have been the efforts to produce the desired apparatus. All the different types, however, proved unsatisfactory for one reason or another, and it is only recently that the latest R.A.E. pattern, which at last seems to fill the bill, has appeared. If its promise is fulfilled, and the indications are optimistic, it must be regarded as a most valuable acquisition.

In other matters accurate assessment of performance is hedged round with difficulties, because owing to the inaccessibility of the engine in the air, it is not at all simple to make even temperature and pressure measurements accurately, and of course one is almost completely deprived of the use of the senses of sight and hearing. Indicators could be used, and flights have in fact been made with the R.A.E. electric indicator, but the difficulties of installation, provision of suitable drives, and manipulation in the air, are so formidable as to quite rule them out for any ordinary everyday use.

Air Testing

From what I have just said, it might be inferred that the art of air testing is in a very elementary state, and probably in many respects this is true, and there is a long way to go before it is reduced to that degree of precision and exactitude that is desirable; nevertheless considerable progress has been made and the technique of some of the aspects of air performance established on a satisfactory basis.

The best example is that known as the radiator or cooling suitability test for aircraft fitted with water-cooled engines.

Probably as much ill-informed nonsense has been talked about radiators as about any other single subject in the aeronautical domain. With the aerodynamic merits or otherwise of the various forms and types of radiators, itself a most complex and perplexing subject, when all the conflicting considerations are fully understood and appreciated, the test is not concerned. It is a test of the means supplied for cooling the engine for cooling efficiency, and enables its suitability to be assessed as a mathematical figure of merit in terms of English summer or tropical summer requirements.

The value of such a test is very real, as it ensures that the cooling will be adequate under conditions which are agreed as the maximum required to be coped with.

It also automatically weeds out the aircraft with small and pseudo highly efficient radiators, which is really under-radiatored. Without some such precise test it is much easier for the under-radiatored craft to "get away with it"

than might be imagined. I do not propose to describe the test in detail, because it is quite simple and in regular use.

With the increasing importance of cowling and streamlining for air-cooled engines a test of a similar, but of course simpler, nature will no doubt become regular routine.

Another direction in which progress is being made is in the analysis of the functioning of carburettors in the air. This subject was dealt with in some detail by Mr. Penn in his recent lecture before the Society in November last, and I shall therefore not attempt to go over the ground again.

It is sufficient, I think, to emphasise that as the outcome of work carried out in the Engine Research Flight at R.A.E., two important steps forward are emerging.

Firstly, the technique of testing new carburettors and the efficiency of carburation is assuming something like a methodical procedure, and secondly, much data are being gathered together as to what are the exact characteristics which are really wanted. Surprising as it may seem to some, in the past there has been a very definite and serious lack of such basic information.

I think I am not far wrong if I say that my impression of the attitude expressed in the discussion on Mr. Penn's paper was that " when you really do know what you want, then carburettor designers can supply the goods." Whether the carburettor designer's job will be an easy one to solve remains to be seen. But it must be all to the good to be able to formulate in a more concrete manner the requirements, and further to see that they really have been met.

Before concluding, there is one other class of performance test in the air to which I wish to refer briefly, as I regard it as of great importance, and it is one which has for many years been more or less shrouded in mystery, owing to the many difficulties surrounding the execution of appropriate tests, and that is fuel consumption.

For a long time there has been good reason, supported by some indirect corroboration, to suspect that considerable divergence might exist between the fuel consumptions, both gross and specific, obtained under the ideal conditions on the test bed, and those actually realised under practical conditions in the air. It was quite clearly appreciated that such discrepancies might arise from two main causes, viz., (i) operational reasons, by which I mean failure of the human element to get the very best possible out of the gear as it existed, and (ii) inherent questions of natural laws and design limitations, etc. An attack on this problem has been much hampered by the lack of reasonably convenient and accurate means of determining the two controlling factors, *i.e.*, the quantity of or rate at which fuel was being used, and simultaneously the h.p. being developed.

The real need for some reliable data on this question being recognised by the Air Ministry, the R.A.E. were instructed to get to grips, and as the outcome of work at Farnborough and in co-operation with Service units, very valuable information has been secured, and suspicions turned into certainties in a striking manner. Particularly was this the case as regards what I might call the shortcomings of the human element. The experiments showed that from this cause engines of the same type operating under closely similar conditions may—I stress the word " may "—reveal the most startling variations in fuel consumption.

When considering the import of such results it is bad logic to comfort oneself by too much contemplation of the best figures. A world, even the aeronautical world, comprises the good, the average and others, and one must cater for all sorts. It is true that in this case, incorrect use, or it is better to say failure, to take the fullest advantage of the control provided by the altitude control was the primary cause of the variations, but the kind of important deduction arising is that for other than the most skilled; what is wanted are

carburettors either automatically compensated or requiring no compensation at all for altitude.

In the second category of effects the issues are not yet so well defined, but as and when data are collected, sifted and analysed, then we shall be able to appreciate them in their true perspective, and I feel fairly confident that facts of important practical value to progress will transpire; for example, I have in mind the oft-debated point of specific fuel consumptions of supercharged V normal or high compression engines under various circumstances and conditions.

In conclusion, I feel that it might be said that this paper has made no reference to any scheme of testing to determine the merit and general performance of an engine as a propelling plant for aircraft.

I have not done so, because I am not optimistic about the chances of any such general test being evolved, and reduced to an exact and ordered procedure, which can be legitimately classed as performance testing. The variables are so numerous and diverse, the requirements under varying conditions so conflicting, and many features are not susceptible under any circumstances to any form of numerical assessment, that any general judgment must in the end be largely swayed by questions of opinion.

In simple straightforward matters, such as the merit in speed or climb, there are difficulties to be surmounted in arriving at true comparisons, although these may not really be very great if one is dealing with a single paramount requirement, for example, say, speed.

For an ideal comparison one would require engines of the same horse-power installed in aircraft specifically designed for the type of engine, but of exactly equal aerodynamic characteristics and merit. Needless to say, such ideal conditions are out of the question, and one is thrown back on the old game of making corrections and approximations, which are all to greater or lesser degrees open to question.

Tests of different engines even in the same aircraft, where this is possible, require the most careful consideration if the risk of false deductions is to be avoided. In the past there has been little or no opportunity of getting together any figures for widely differing types, such as water-cooled or air-cooled engines even in the same machine, but of course there is now a certain well-known aircraft engined at choice with either three types of water-cooled or one type of air-cooled engines, which should produce some interesting comparisons.

If there are other matters, such as air testing of supercharged engines, a large and tough lump of meat requiring separate treatment for adequate digestion, which it is felt should have been discussed, I must ask your indulgence on the plea of the limitations imposed by a paper of this general character, and I would suggest that the discussion affords the requisite opportunity for putting them forward.

Discussion

The Chairman (Major Bulman): He was sure they would agree that the object, which Wing Commander Hynes had in preparing the paper, of inviting a wide discussion, had been achieved. He had provided an extraordinary number of tests from which discourse could emerge, and a large number of pegs on which various bonnets could be hung.

Mr. A. H. R. Fedden: First of all, dealing with ground testing, he would like to say, speaking with some feeling, that Wing Commander Hynes and his colleagues must be congratulated on the excellent methods which had been established. He had had experience of type tests in a good many countries with their own engine, and he could say emphatically that there was no type test which was so clearly defined and laid down, and in which one knew so exactly what had to be done.

If one might make a suggestion with regard to the Test Schedule, A.P. 830, he would ask Wing Commander Hynes whether he would approve of different ways to increase the severity of the overload test.

Although the American type test was only 10 five-hour periods, he was informed that the five hours at full throttle maximum r.p.m. was apt to find out weaknesses more consistently than our type test at 100 hours at 90 per cent. of normal power, with one hour at 10 per cent. overload.

He felt that the question of defining the cylinder temperature for bench tests did want some further consideration. He knew that this figure had only been raised as an interim measure quite lately, but he had had experience of an engine installed in different machines in which the temperature variation was as much as 100 degrees.

He would ask Wing Commander Hynes whether he did not think that this definition was rather unfair to supercharged engines, because this must definitely affect the detonation test, if it were to be under the most severe conditions.

Dealing with the R.A.E. indicator, he would like to ask Wing Commander Hynes whether he thought that one could get true indicated mean effective pressures with that indicator. He had been trying to do some tests lately at revolutions of 2,000 and above, and had found that it was very difficult to determine accurately the peak pressures. There was a good deal of variation in the curves, in spite of the utmost care being taken.

With reference to the R.A.E. flowmeter, of which within the last two weeks he had had some experience, as far as one could see at present it seemed to be accurate within 2 per cent. He thought it was an excellent instrument, which would fill a long-felt want.

With regard to supercharged engines, the greatest corrections were due to temperature. He had found that care must be taken to see that the inlets to the air boxes, fitted to produce the required depression in the air intake, were kept clear of the engine, so that the air intake temperatures would not be affected by hot gases.

One rather interesting case that he had come across recently, where the test had been taken with great care, the air intake temperature on the one side of the box was 27 degrees Centigrade, and on the other side, taken at the same time, it was 10 degrees Centigrade. That raised the point that, though temperature correction was important, it was equally important to take care as to the position at which the temperature was taken.

In the Addendum to A.P. 840 the various corrections did appear formidable in respect to supercharged engines. If, however, the combined correction factors were plotted for various intake temperatures and boost pressures, the curves obtained were straight lines, and with the aid of those curves the corrected b.h.p. at certain altitudes could be read off direct. This scheme had been found to be most useful for testing purposes. It would be noted that for intake temperatures up to about 24 degrees Centigrade the b.h.p. correction for one degree Centigrade was greater than the correction for a difference in observed boost pressure of $\frac{1}{2}$ lb. per square inch. He did not know whether Wing Commander Hynes could give them any views on that matter.

Also he would like to ask Wing Commander Hynes for his views as to the effects of intake temperature on detonation, and upon power development at the useful limit of detonation. He felt that the question of airscrew dynamometers was one of very great importance, and Wing Commander Hynes had told them about the only one of which he knew. Could Wing Commander Hynes tell them whether he thought there was any prospect of getting an airscrew dynamometer in the near future, or did he feel that the technical development in the work of producing such an instrument was going to be a very long and tedious job, because, if so, he thought it was one of the matters that the Air Ministry should push on with?

Major GREEN: He was glad to see that Wing Commander Hynes made a distinction between testing for performance and routine tests. That was a valuable distinction that was often overlooked. He agreed with Mr. Fedden that it was not logical to take the worst possible case in flight and to run the engine on the bench at the temperature so obtained. It would depend too much upon the installation in the aeroplane, and particularly upon its speed. It would not be altogether unreasonable to have not one type test of horse-power but several, according to the sort of aeroplane into which the engine was to be installed. On a very fast aeroplane the cooling may be so much improved that it would be possible to get more horse-power than on a slower machine.

It was well known that the temperature in flight and on the bench varied considerably with the mixture strength. Weak mixtures, particularly, always meant hotter cylinders. It was difficult to say what was the workable petrol consumption without knowing exactly the conditions of flight. The high speed aeroplane could probably be worked on a lower petrol consumption on account of its better cooling than a slow speed aeroplane. If the figures for test temperatures were taken from flying tests then it would also be necessary to specify petrol consumption during that test.

In actual flying he thought that petrol consumption depended much more upon the proper use of the altitude control and on good installation than on the type of engine used. He agreed with the lecturer in welcoming a means of taking petrol consumptions quickly and accurately and was glad to know that the flowmeter was now an accomplished fact.

On one particular point he wished to ask Wing Commander Hynes for further information. It was suggested that the temperature of the engine on test should be taken at the sparking plug boss and that, failing flight test, an arbitrary figure for the temperature of the engine was to be 150° Centigrade. This seemed to lead to the rather absurd siuation that if one wanted to get the easiest test conditions for a particular engine then one would put the sparking plug in the hottest place. This would mean that the average temperature of the cylinder would be lower when using the method of temperature measurement suggested. If, on the other hand, one put the sparking plug in the coolest place then the average temperature of the cylinder on test would be very much higher.

He would also like Wing Commander Hynes to give them some explanation of the difference of detonation in an engine which was throttled by means of a valve and one which got air at similar density on account of reduction of pressure of the atmosphere. Unless it was caused by back pressure or temperature he could not see why there should be any difference, and some explanation of that point would be useful.

Finally, Wing Commander Hynes stated that the ideal thing would be to get the actual effect of the power unit on an aeroplane, and suggested that this was almost impossible. The speaker took a more hopeful view. In America there was a 20 ft. wind channel in which an aeroplane body complete with its engine could be placed. It seemed quite a simple matter to run the engine and to measure the thrust of the airscrew direct and the average air speed in the channel. These, when multiplied together, would give the actual thrust horse-power of the engine and the over-all efficiency measured against petrol consumption, which were the facts we really all wanted to know. He thanked the lecturer for his very valuable paper.

Captain SWAN: Wing Commander Hynes had explained clearly that the ground testing of a normal engine was on a firmly established basis; but when they came to supercharged engines, the present method with regard to testing was of necessity on a very tentative basis. Many corrections were required before they got what was called rated full power, which might or might not equal the power developed in flight. The difficulties of simulating the conditions

of flight in ground testing of supercharged engines were considerable, and probably impossible commercially. That emphasised very strongly the necessity for some form of dynamometer, or meter for measurement of power in the air. He rather anticipated that what Colonel Hynes described as the "air apprenticeship" of an engine, which at present was intended to eliminate mechanical defects and at the same time test the cooling system and other subsidiaries, would be extended in the case of supercharged engines to cover much more comprehensive tests. The development of the flowmeter had been a big step towards that end; its accuracy in flight was proved and its use would tend to economical flying. Unfortunately, in experimental work, it could not be used to the best advantage at present, because of the lack of a power meter. When the consumption obtained with the flowmeter was reduced to specific consumption, using the estimated power, the results obtained added to their perplexities.

The National Advisory Committee for Aeronautics of America had done some further tests using a hub dynamometer, and the results were fairly consistent, although the absolute values were still in doubt. The unfortunate part was that there was nothing to compare the results with. A curve had been plotted using theoretical values founded on a fairly reasonable and rational basis, and the divergence between the experimental results and the theoretical curve was not wide. He mentioned that several designs of dynamometers were under investigation and he was not altogether without hope that a device could be made of reasonable accuracy for the purpose.

Another point was the complication in supercharged engines of using two boost pressures, a normal and a maximum pressure. To use two boost pressures in aircraft required the introduction of an extra device on the throttle. The present simple method of operation of the throttle, which could be retained if one boost pressure only were used, would be lost and he doubted if the small advantage of using two boost pressures was worth it, especially as the difference between the normal and maximum boost pressures was only a matter of $\frac{1}{2}$ lb. per sq. inch. In flight the combination of maximum boost pressure and maximum revolutions, *i.e.*, maximum power, could only occur in two cases, namely, level flying at full throttle height, and aerobatics, so that if one boost pressure was adopted, that being the maximum allowable or something very near to it, the loads imposed on the engine would not really be appreciably greater than if two boosts, normal and maximum, were used. In addition, complication in the installation would be saved. The device fitted to permit normal and maximum boost to be used as and when required might be operated correctly in practice flights, but would probably be disregarded under actice service conditions and the engine would then be used at maximum boost throughout.

Mr. W. L. Taylor: He would like to make one remark in connection with the empirical formulæ used in connection with Addendum A, namely, that those formulæ had been built up on the mean of test results obtained from various types of engines. It did not seem possible to get a formula which applied equally to all engines. The positive "back pressure" correction was a figure which, for the several engines tested up to the present, did not vary very much, but the correction for negative "back pressure" figure did vary, mostly on account of the different valve overlap obtaining on different engines. Then, if he might go back to the question of the brake, Wing Commander Hynes had made no reference to the electric dynamometer. The present form of Froude hydraulic brakes and the earlier brakes involve a great waste of power. The whole b.h.p. of the test engine is put into water, which is allowed to run to waste down the drain. If an electric dynamometer were used the power house load could be boosted, thus helping considerably towards cheapening the cost of testing.

Mr. H. B. Taylor: Mr. W. L. Taylor had just taken the words out of his mouth, as he was going to refer to electric dynamometers. There were not sufficient people who realised how much information could be obtained from an

engine without running it under its own power. He would like to know what Commander Hynes thought in that respect, because he did not mention it at all as a piece of equipment. It had many merits to recommend it over the other methods which Commander Hynes had mentioned. There was certainly a big waste in testing in factories and the use of electric dynamometers would reduce the power absorbed by the running of the plant and in some cases would actually improve the power factor and should reduce the electricity bill where the cost per unit was a function of the power factor. There was one other point, namely, with reference to the washer type of thermal-couple which was used under the sparking plug. His view was that it was not reliable and the temperature recorded would vary very largely with the amount of gas leakage at the plug. Some plugs were very bad, and some were very good. But one might get a bad plug and then would record a hot cylinder, though one really had not a hot cylinder. He would welcome any information that Commander Hynes could give them as regards the use of a fixed or permanent type of couple which could be used in any part of the cylinder. That was a subject to which some attention should be paid. He knew of no type of couple which was really reliable, and which could be used in any part of the cylinder and left fixed in the cylinder so that it could be used in flight.

Captain Dobson: He was afraid he could not say much on the general engine test because he was more particularly interested in the carburettor and carburation. A great deal of the paper had necessarily dealt with the engine as such. When one considered the very wide gulf which undoubtedly existed between the results achieved on the bench and in flight he was led to believe that that was very largely due to the question of carburation. As regards the induction system on the bench, these were almost universally tested in a hot condition and every effort was made to maintain the optimum temperature. Unfortunately, under flying conditions, that optimum temperature was seldom attained, and very often, especially in a dive, the induction pipe must function quite cold. The effect of poor distribution obtained under these conditions was much greater than a mere matter of effecting power or fuel economy. It could have a very bad effect on the valves, the sparking plugs, the pistons, cylinders, and so on. If the induction pipe were designed and tested to work while cold the flying life of the engine would probably be very much prolonged. That was not just an academic wish. He recently had had the opportunity of designing and constructing such a pipe for an aircraft on which the distribution was excellent even when running cold. It was a little dangerous, because one could fly the machine straight away without having to warm up. Another cause of the discrepancy between the bench and flight results was the effect of air intake alterations when the pressure balance system was not perfect. Those would have just the same damaging effect on the engine as the previous distribution troubles. But when the balance system and the carburettor had been properly arranged it had recently been proved that it was possible to make very radical alterations to the air intake of a particular engine without affecting the performance or consumption in the air, and without requiring any alteration of the bench set of the carburettor as found in the dynamometer. He was not sure whether one would be permitted to make that type of pressure balance in many cases as it involved the introduction of a bit of external piping. From those instances of personal experience, it appeared to him that part of the routine of the bench test should consist of proving that the carburation was at least reasonably good under cold conditions and by means of air blast into the carburettor intake to see that the pressure balance of the carburettor, or the carburettor system generally, was capable of coping with an air blast which would arise in many different ways. Commander Hynes had made no mention of a throttle curve being taken. It was a comparatively recent innovation, but he believed it was to-day the standard procedure. He thought it was a very useful one, because it was the only

bench test which showed actually what sort of consumption was likely to be obtained when an engine was running throttled. It was hardly necessary to stress the fact that the majority of aircraft had a relatively short time flying full throttle, and that cruising speed economy was of very great importance. In that respect, it was no use stressing the fact unless one knew how to cure the difficulty and, therefore, he was just going to give them a little propaganda. It was essential that the mixture of maximum power should be richer than that which gave the maximum economy, and it was also necessary to have engines rich enough to prevent undesirable hot conditions of plugs and valves at full throttle; whereas when running ordinary throttle the leaner mixture could be used for economy without detriment to the already mentioned hot stressed parts. That was accomplished by a power jet arrangement of which they were pioneers. With regard to the statement made in Commander Hynes' paper that carburettor designers could supply the goods when they were told what was really wanted, he did not think that statement went quite far enough. They had actually been saying for some years now that they thought they knew what should really be wanted and were ready to supply it in most cases when permitted. In regard to the discrepancy in petrol consumption between machines flying under similar conditions he agreed, so far as his knowledge of the pilots was concerned, that the majority of them either misused altitude control or did not use it at all. But it was not their fault. It had largely come about because the altitude control methods generally in use were very tricky and dangerous sort of things because they did not display the whole throttle curve parallel; they had more effect on one of the curves than on the other.

Major Ross: He had come with the idea, not of joining in the discussion, but rather in the hope of hearing something which might help him to modify some of his preconceived ideas.

There were one or two matters that he might comment upon. The first thing he had noticed in the paper was the description of the Heenan Fell type of brake, as being what is known as a "wind stick." He did not think the makers would quite appreciate that description because the "wind stick" type of Fell brake had gone out of use for some time. The Heenan Fell fan consisted of a rather heavy and accurately balanced mild steel plate rotor of many blades.

Dealing next with the question of the temperature of air-cooled cylinders, from the remarks of previous speakers, there appeared to be the same sort of misunderstanding with regard to the use of 150°C. as a basic temperature as had existed in the past. The official position appeared to be that where one had no knowledge of what was the normal temperature of a new cylinder design, the first bench tests of the engine should be started with air-cooling conditions adjusted to give the purely arbitrary temperature of 150°C., so that the authorised inspectors had something to work to.

This temperature would subsequently be raised or lowered as the case might be as soon as actual flight experience had been obtained. He admitted, however, that as other speakers had said, the temperature established in flight might be affected by the petrol consumptions and the type of aircraft, but the figures obtained had always had to be used with quite a measure of discretion and caution.

With regard to the method of measuring the temperature, he would like to see some provision made definitely on the air-cooled cylinder for the attachment of a reliable thermal couple such as, for instance, by providing a small drilled boss on the cylinder head in a representative position. If that were done, and a suitable thermal couple inserted, one would get much more accurate results than from the present sparking plug washer couple.

The author had suggested that reasonably accurate comparative figures could be obtained with the sparking plug washer couple. His experience, how-

ever, was that the use of this couple could tend to very misleading results, unless special precautions were taken. Many variations from normal figures had been tracked down to misuse or the method of fitting.

This couple often only indicated the heat flow between the cylinder head and the sparking plug, and on some engines it might be necessary to insulate the couple from the plug to prevent an overheated plug giving very hot readings. That was one reason why he would like to see better arrangements made in the air-cooled cylinder head for the fitting of a buried type of thermal couple. The pencil type of couple has been used with satisfactory results on some engines, but its use was open to very much the same objections as the washer type.

With regard to Addendum A. of A.P. 840, which the author described as "liberally fitted with cabalistic signs," the supercharger engine introduced new difficulties in establishing a basis for definite standards to compare performances at full throttle, and also in providing a means of maintaining the same standard of performance on engines of different types.

In dealing with the testing conditions to represent the relative performance at altitudes, one rather ignores, for example, the variations in mechanical efficiency between individual types of engine, and assumes an average. Water-cooled engines being about 88 per cent. and air-cooled engines 92 per cent., a mean of 90 per cent. is taken to cover all types.

In the same way with the conditions for the supercharger, one had to strike a mean of different types as the result of tests and say that the same form of blower had the same average efficiency, were similarly affected by the exhaust back pressure, and also the air intake temperature.

Empirical formulæ based on these assumptions could then give a comparative standard of accuracy, but the result was that an engine which had poor blower efficiencies might be credited with a better performance than it would do at its full throttle altitude, whilst an engine of good over-all efficiency would not necessarily get full credit on its log sheet for the performance it would give.

He thought that these discrepancies levelled themselves out in the actual performance in the aircraft, the better type design giving the estimated or better performance, whilst the poor design would not come quite up to expectations, and would therefore draw attention to its failings.

As regards performance testing in the air, he was particularly interested in the method of making comparisons between aircraft with the same type of engine and the respective performance of this engine on the test bench.

In making these comparisons, the horse-power given in the log sheets of the test bench power curves was usually taken as a basis of calculation, but the variation in the conditions under which these figures were obtained and those in the air were often ignored.

On the bench the observed horse-power was corrected for exhaust silencer back pressure, air intake temperature, and barometric pressure, to what were in some cases only empirical formula.

When the engine was installed in the aircraft, the exhaust back pressure and air intake conditions were different, but it was not usual to find that corrections had been applied to the basic power figures, or if they had, it was just as likely that some different formula had been used. Considering these possible errors combined with the presumed accuracy of revolution indicators and other recording instruments, it is not surprising that estimations of the power available at altitude often varied as much as 5 to 10 per cent., but on the assumptions made they were equally accurate.

He hoped to hear further discussion on these points, particularly relating to the estimated accuracy of characteristic airscrew calibrations as compared with actual engine power curves and the value of any corrections applied even although they might be purely arbitrary.

Mr. BRAMSON: There seemed to be general agreement that it would be a very great step forward if power could be measured with reasonable accuracy in flight. Had it been considered whether an estimate of power could be made on the assumption that the mechanical efficiency of any given engine did not vary very much as between bench and flying tests; and whether in that case indicator diagrams could be taken in flight simultaneously with other readings so that satisfactory comparisons could be made? He did not know how difficult it was to take indicator diagrams in flight, but perhaps the lecturer could tell them about that. A great incidental advantage of measuring power in flight would be that the measurement of the propeller efficiency in flight would be so much easier. It appeared well nigh impossible to get hold of a really true propeller efficiency in flight. Quite recently he had faced the difficulty, and there did not appear to be any practical way of establishing an absolute efficiency figure for any given propeller apart from comparative flight tests. As regards altitude control, the pilot was rather in a fix, because if one tried to use the altitude control skilfully or carefully, one found almost invariably, whilst concentrating on the revolutions indicator, that meanwhile the air speed had varied, and so one could not find out exactly what was the optimum position of the altitude control.

He would ask (hoping the question would not be considered irrelevant): Had it been established in the course of the tests of which the lecturer had wide experience, whether there was any fundamental reason why air-cooled engines should have worse consumption figures than water-cooled engines? Did it lie purely in the fact that higher compression ratios were permissible; or did other factors affect the question?

Finally, he would ask the lecturer whether it was absolutely impossible to establish some sort of figure of merit, taking into account, for instance, power weight ratio, specific consumption, and horse-power per unit frontal area? Would not it be possible to publish a figure of merit for engines intended for aircraft within a defined range of type and speed which would make them comparable and capable of being judged on suitability?

Mr. W. PATRICK D. BENNETT (*communicated*): He was surprised no one had mentioned an alternative to the hub dynamometer or the determination of torque from airscrew and/or aerodynamic data. He suggested that the whole engine and mounting could be arranged to rotate within small limits about the airscrew axes. The torque reaction could be measured either by balancing it against springs or hydraulically. The practical details of the construction of such an apparatus should not present any difficulties. The only correction to be applied would be for losses due to mechanical inefficiencies such as friction. The only actual measuring instrument would be a pressure gauge. The torque would then be found from the known area and number of pistons, moment about axis, etc. Hydraulic pressure could be supplied by a wind-driven oil pump, the pressure being varied by an adjustable bye-pass valve. The movement would be regulated to a couple of degrees or less and might be made so that it could be locked when tests were not being carried out. An electrical apparatus could be arranged so that when the oil pressure torque exceeded the engine torque, a red light would show, and a green light when the pressures were reversed, no light showing in a state of equilibrium.

REPLY TO DISCUSSION

Wing Commander G. B. HYNES: Mr. Fedden had discussed the question of the relative values of different forms of overload test. That was a matter capable of giving rise to a tremendous amount of argument, but the impression he had been given was that he (Mr. Fedden) was in favour of increasing the severity of the overload test. Coming from a constructor he (the speaker)

welcomed that suggestion. The more overload an engine would carry up to a point, the better; but it was an exceedingly difficult thing to know exactly what the relation was between a certain amount of overload for a certain time to another amount of overload for a different time. It was a subject which was open to any amount of discussion. The same sort of problem arose in other walks of life. As a matter of fact, when he was a Gunner, he had had experience of a similar class of thing in regard to the amount of wear taken out of a gun by firing it. The ordinarily accepted comparison was that four three-quarter charges only did as much damage as one full charge; while 16 half charges only equalled one full charge. He thought that was an analogy. Whether one overloaded an engine 10 per cent. for a certain time, or whether one overloaded it rather less for a longer time, it was very difficult to answer offhand what would be the comparative result. The second point Mr. Fedden raised was whether it was fair to the engine that the temperatures should be recorded in the worst conditions of flight. There was more than one way of looking at that query, but so long as the Ministry wished to have engines of fairly universal application, one must accept it that the engines must be fit and suitable for the worst duty which they were likely to be required to perform.

Major Green suggested alternative ratings, according to the class of machines that the engines were going into, but such a course was open to a number of practical objections. He would suggest there was another way of looking at the matter. If an engine was efficiently cooled to an extent that it would remain cool under the worst conditions of flight, then as one improved the conditions of flight and put it into higher speed machines, one could proportionately increase the amount of cowling and improve the streamlining. Therefore, the difference in power, due to the variation in cooling, over wide ranges of speed would not really be quite so serious as might appear at first sight. From the practical point of view he thought that it was better to look at it in that way, namely, to recognise that a well cooled engine could be more and more cowled as it went into high to very high speed machines than to advocate a lot of alternative ratings.

More than one speaker had asked about the reliability of indicated mean pressures and horse-powers as obtained by such means as the R.A.E. indicator. He felt that they must recognise that the indicator was not a reliable or accurate method of measuring power. It was wrapped up with difficulties of phasing the indicator. There had been a great deal written about that matter, and a great deal of work had been done at the Air Ministry Laboratory on the subject of phasing indicators. He believed that Mr. Ricardo's conclusions after lengthy experience were that with the greatest care one could not rely on a degree of accuracy greater than about 5 per cent., and even this was difficult to achieve. As regards the question of the actual pressures recorded, he did not quite understand what the difficulty was about that. There seemed to be some suggestion that the results were not uniform. He could only suggest that that was some minor fault in the apparatus or installation or in the conditions of operation, because when the indicator was working accurately, it was one of the outstanding features that the results were remarkably uniform, unless it so happened that actually the engine was in fact running under conditions of very variable maximum pressures. That was a possible solution of the apparent discrepancies. As to the question of the effects of detonation, he would have to leave that, as the whole subject was so very controversial. As to the airscrew dynamometer, that was mainly a question of emphasising what he had already said. Work was proceeding, but he would not care to hazard any prediction as to when success might be achieved. He had answered Major Green's first point by suggesting that it was a question of policy whether one was going to have the same type of engine rated at different powers, or whether one would take advantage of closer cowling. As to petrol consumption and weak mixtures,

what had been said was, no doubt, correct. If the engine were run under very well cooled conditions, one could run with weaker mixtures. That, of course, did have a bearing on the petrol consumption; but it was rather difficult to assess it so that it would be of practical utility. He did not think one must take too seriously the idea that designers were going to put restrictions on the design of an engine with the sole purpose of getting the sparking plug in the best position to defeat the requirements of the test schedule. It was by no means essential that the sparking plug washer type of thermo couple should be used; as Major Ross had said, very rightly, it was only a temporary and convenient expedient. The whole matter of these cylinder temperature measurements was in a tentative state. As regards the difference between the power developed by throttling high compression engines and by reduction of atmospheric density, the statement made was simply a matter of fact. There were possible explanations which he could not go into, but it was a well-established and well-known fact, which had been proved by experimental running at Farnborough, that the effect on power of the reduction of charge density by throttling was not exactly the same as that due to reduction of air density. He was very interested to hear that Major Green was hopeful of getting a comparison of complete power plants; but even then there still remained a serious obstacle to a strict comparison in the time sense. Supposing one took an aircraft designed round a water-cooled engine and put it into a wind tunnel and got its characteristics, then without any fundamental change, he did not think one could say that this same aircraft was the best possible design, if one wanted to put a radial engine into it. It was a point on which he did lay a great deal of stress, as it was one of real difficulty. If one wanted to compare two widely differing types, like an "in-line" water-cooled engine and a radial engine, he would point out that the two machines required to take those two engines were so different that it was very hard to ensure that they would both have the same fundamental characteristics and be of exactly equal aerodynamic merit. He agreed that his first suggestion would go some way along the road, but he was not sure it went as far as was required. Then another speaker referred to the fact that the merit of the power plant was not solely a question of thrust and efficiency. This was very true and there were all sorts of things which came in in assessing the merit of a plant such as, from the Service point of view, maintenance and suitability for the field, and also manufacturing questions which had to be taken account of. If one were concerned with only one or perhaps two requirements which were of paramount importance, such as speed at any price, then a comparison might be of use. He had been interested to hear from Captain Swann that there would be an extension of the use of the hub dynamometer; but he was afraid that that particular type would never go far because of its clumsiness and obstacles to easy installation. He wished they could see more signs of something else coming on. As to the point about boost pressures, he must leave that because it was rather a complex argument, but nevertheless it was appreciated that it was certainly open to debate

Mr. W. L. Taylor had referred to electric dynamometers; the omission of a reference to this type of brake was certainly an oversight, as they are very convenient. The larger beds at Farnborough for bench testing are a mixture of the Heenan, Froude and electric types. He agreed that the electric dynamometer has advantages which Mr. Taylor had mentioned, but he had had no personal experience with the Heenan Highfield type.

Mr. H. B. Taylor's remarks about running engines not under power was rather a border-line case as to whether it was within the terms of reference of the subject of the paper. The difficulty in a paper of such wide scope as this was not to think of all the things one might refer to, but much more to keep as strictly as possible to the terms of reference. He quite appreciated his point about the gas leakage affecting the thermo couples; no doubt he should have

qualified the reference to them by saying that these couples were only satisfactory when they were very carefully installed and used. What was wanted, as Major Ross had said, was a better type of couple, with suitable provision for fixing it.

Major Dobson had verged rather on questions which were again outside the terms of reference; but incidentally, he would mention that this matter of the reproduction of cold conditions on the test bench was going to be rather controversial and expensive, and it was probably better to rely on the flight trials. He accepted Major Ross's correction about the wind stick; but he thought he was already covered in the paper, because he had said, "two or more arms," and the ordinary person knew that as a wind stick. He accepted the suggestion that better provision for fitting thermal couples would be an undoubted improvement.

As regards the question of estimation of performance in the air, he could not give them quite what they were hoping for. It all pointed to that what was really wanted was a dynamometer by which one could measure power in the air.

Mr. Bramson's first question was really covered by what had been said about the R.A.E. indicator as a method of power measurement. If one could not rely on getting the indicated horse-power right, the question of mechanical efficiency did not really arise. His second point was again to emphasise that a dynamometer was wanted to get propeller efficiencies. As to the petrol consumption figures for air-cooled and water-cooled engines, without going into a lot of detail, one must take it that where the conditions are comparable the petrol consumption of the water-cooled engine was generally rather better than that of the air-cooled engine. There were a good many reasons why this should be so, and the fact remained, though the advocates of the air-cooled engine would not agree with him, perhaps, that the air-cooled consumptions were usually higher than the water-cooled consumptions. The question of a figure of merit revolved round the difficulties already alluded to, namely, that if one was concerned with one or two special features only, possibly some formula might be found; but he did not see how they were ever going to assess any sort of empirical value to such things as ease of production and Service suitability, or even to such matters as the figures for consumption, because, according to th different purposes for which a power plant was required, these various attributes would take on different values. But, as previously stated, no doubt something could be done if one was only concerned with one, or possibly two, specific requirements. He was sorry that the last speaker disagreed with him in his interpretation of the title of the lecture. That was a matter of opinion. He was dealing with the question as he saw it, and many of the points that that speaker had raised were really only a different way of describing the same end to which they were all working, that of producing the best and most reliable engines.

In reply to Mr. Bennett, the suggestion put forward is not new and, in fact, an aircraft was fitted and flown at the R.A.E. with a torque reaction mounting. The results obtained, however, showed large errors, the reasons for which are quite understood, and in consequence the scheme was not pursued further.

PROCEEDINGS

NINTH MEETING, SECOND HALF, 64TH SESSION

In the Chair:

THE PRESIDENT, COLONEL THE MASTER OF SEMPILL

A meeting of the Royal Aeronautical Society was held in the lecture theatre of the Royal Society of Arts, 18, John Street, Adelphi, London, W.C.2, on Thursday, April 18th, 1929.

The PRESIDENT: Lieutenant-Colonel Richmond lectured before the Society in 1921, 1924 and 1926 on various problems concerning airships. As the officer in charge of airship design and research at Cardington he was responsible for the R.101, which was rapidly approaching completion, and he had asked him (the President) to express the indebtedness of himself and his staff to those concerned with heavier-than-aircraft, and to Mr. J. D. North in particular for their valuable co-operation and assistance in connection with the design of this ship.

A lecture concerning airships always attracted a large audience. Lieutenant-Colonel Richmond was to be congratulated upon the great work he had been engaged on at Cardington, and, whilst he would welcome any criticism, one deplored the fact that he and others concerned with the development of airships had been subjected to too much uninformed criticism.

R.101

BY

LIEUTENANT-COLONEL V. C. RICHMOND, O.B.E., B.Sc., A.R.C.S., A.F.R.AE.S.

Certain details of the airship R.101 (now under construction at the Royal Airship Works, Cardington) have already been given in technical papers and general descriptions have appeared in the Press. References to these will be found in the course of the present paper and it is hoped to avoid mere repetition. The aim is, as far as possible, to give now a connected account of the principal features of the ship and the reasons for their employment. Space does not permit of a description of the actual *processes* of design and construction though, doubtless, this may be possible later.[1] The design of R.101 is quite unconventional though some of the changes may only be apparent to those who are closely familiar with the design of past rigid airships. Were it not for certain criticisms which have appeared, it would be unnecessary to point out that these departures from conventional Zeppelin practice have not been made for the sake of mere change nor lightly undertaken. Some have been dictated by the great increase in size of the airship. Others represent progressive development, which, though not perhaps making itself fully felt in the first airship of such a novel type may well prove of first class importance in the future. The production of new materials with improved mechanical properties also leads to changes in struc-

[1] *e.g.*, a paper by Mr. H. R. Cox, of the Staff of the Royal Airship Works, on "The External Forces in an Airship Structure, with special reference to the requirements of Rigid Airship Design," won the Society's R.38 Memorial Prize last year, and will be published shortly.

tural design, as for example the duralumin ships of Zeppelin (the excellence of the design of which is ungrudgingly acknowledged) have superseded the wooden ones of Schütte-Lanz.

Aerodynamic Characteristics

A complete and extensive series of progressive aerodynamic experiments was planned in the autumn of 1924. This was eventually carried out in its entirety by the Aerodynamics Department of the National Physical Laboratory, being completed by the end of 1926. The fact that these experiments were more complete and comprehensive than had been attempted before for any airship in this country will be gathered from the following list:—

(1) The measurement of drag, lateral force, yawing moments and damping coefficients on two bare hull forms of circular cross-section.
(2) Tests of 11 alternative shapes of fin on these forms.
(3) Measurements similar to (1) on a hull of polygonal cross-section fitted with the best form fins selected from (2).
(4) Distribution of pressure over the hull.
(5) Distribution of pressure over the fins and control surfaces.
(6) Hinge moments on the control surfaces as used in (5) and on modified forms.
(7) The magnitude and direction of wind speed near the hull.
(8) The drag of a model power car with rotating and stationary propeller and with alternative arrangements of the radiator.

The author desires to acknowledge most gratefully the valuable work done by the staff of the Aerodynamics Department of the N.P.L. in carrying out these experiments and also the helpful advice given by them. A full account has been published,[2] and it is only possible within the scope of the present paper to indicate the more salient points of interest and novelty.

The general shape of the hull which may be inferred from Fig. 2 is one of lower fineness ratio than has been employed in previous rigid airships (5.5) and it has no parallel portion. It approximates closely to a shape (known as U.721) evolved by the National Physical Laboratory during the war and found to have a remarkably low drag coefficient, *i.e.*, little more than 2 per cent. of the resistance of a circular plate of the same diameter as the maximum diameter of the form. The drag coefficient of the bare hull of R.33 (which is typical of the best of the older shapes) in the wind tunnel is 0.0115, that of R.101 is 0.0070 with circular cross-section and 0.00725 with polygonal cross-section of 30 sides.

In ships of low fineness ratio without parallel portion, such as R.101, the curvature of the longitudinal girders cannot be neglected if severe discontinuities in the outline are to be avoided at the transverse frames. Even in R.33, the girders of which are made straight, the effect of these discontinuities was noticeable from the full-scale pressure plotting experiments.[3]

In R.101 it is found that these longitudinal girders, however, can be made to conform to the arcs of circles without departing from the theoretical contour by more than a few millimetres on full-scale. The girders are all accurately finished off to length with end forks before erection and are finally bolted in place. This, together with the question of their curvature, requires most accurate geometrical calculation, and for the purpose it is necessary to make the profile of the ship conform to some definite equation. The equation for R.101 is

$$y = \{ 0.05549\,(400 - 0.00076\,x^2)^{\frac{1}{2}} + 0.09968 \}\,[x\,(20 - 0.02762\,x)]^{\frac{1}{2}}$$

x = distance from bow in ft.
y = ordinate or radius in ft.

[2] *Vide* Aeronautical Research Committee's Reports & Memoranda Nos. 1168 and 1169.
[3] *Vide* Aeronautical Research Committee's Reports & Memoranda No. 1044.

A method for evolving equations of this type has already been published.[4] This curve shows no sudden changes in slope or curvature (*i.e.*, in the graphs of its first and second derivatives), the latter decreasing progressively towards the tail.

The question of the probable stability of forms of low fineness ratio has been extensively discussed.[4]

The extensive wind tunnel tests indicated above give every confidence that with the proper design of fins these forms are easier rather than harder to stabilise than the older forms. This controversy must now await the test of actual flight.

The experiments on different shapes and positions of fins proved extremely interesting. The more important alternatives are indicated in Fig. 1. The R group are more or less conventional in shape, the T group are shorter and wide and approximately triangular. The first experiments were carried out with thin flat fins. Of the R group, R_1 is the least efficient and R_4 is the most efficient.[5] R_1 is about equal in efficiency to the flat fins fitted to R.33. The T group is, however, considerably more efficient than the R group, T_3 being the most efficient of all. The final choice of shape and position was therefore based on T_3. The

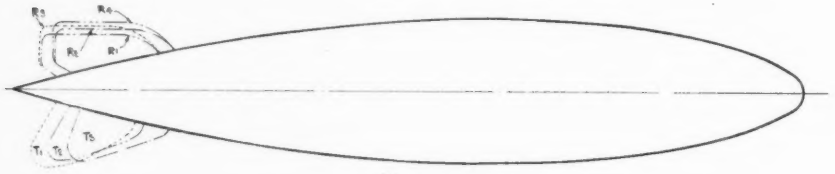


FIG. 1.
Experimental profiles for fins.

fins of previous rigid airships have either been of the thin flat type as exemplified by R.33, requiring excessive external bracing for their support with its attendant objectional drag, or they have been of the cantilever type with V cross-section, requiring very little bracing, as exemplified by R.38. The wind tunnel experiments on these two shapes showed, however, that the fins of the latter were far less efficient than those of the former. It was desired, however, to fit fins of the cantilever type to R.101 for structural reasons and to avoid the drag of external bracing, so that the next experiments were with fins (F_1) of V cross-section of approximately the same shape and area as T_3. These proved more efficient than the V fins of R.38, and stability considerations showed that they were unnecessarily large. Doubtless some of the improvement was due to the better flow of air over the hull of R.101. The four fins F_1 increased the total drag coefficient from 0.00725 to 0.00985. In the next experiments, concurrently with some reductions in area, an attempt was made to reduce the drag by making the longitudinal sections of the fins correspond as nearly as possible with Joukowsky profiles (F_2). This was eminently successful. Not only was the total drag coefficient reduced to 0.00885, but the efficiency of the fins was unexpectedly increased so that finally a further decrease in area was necessary (F_3). The results are summarised in the following table:—

[4] *Vide* "Some Modern Developments in Rigid Airship Construction," V. C. Richmond, Inst. of Naval Architects, March 30th, 1928.

[5] The criteria taken for fin efficiency at a given angle of yaw are:—

(Lateral force on hull and fins—Lateral force on hull alone) $/\rho V^2 A_f$

and

(Moment on hull and fins—Moment on hull alone) $/\rho V^2 A_f \lambda$

A_f = Area of fins and flaps.

λ = Distance of centre of area of fins from the C.B.

Fin profile.	Cross-section.	Area of two fins on model (sq. ft.).	Efficiency* 4° yaw.	Efficiency* 10° yaw.
R_1	Flat	0.1138	0.075	0.248
R_2	,,	0.1252	0.085	0.268
R_3	,,	0.1440	0.081	0.260
R_4	,,	0.1513	0.091	0.282
T_1	,,	0.1400	0.117	0.308
T_2	,,	0.1149	0.111	0.320
T_3	,,	0.1190	0.114	0.329
F_1	V	0.1180	0.093	0.270
F_2	Joukowsky Sections	0.1077	0.103	0.263
F_3	Joukowsky Sections	0.0944	0.109	0.292
R.33	Flat	—	0.071	0.258
R.38	V	—	0.034	0.153

* Efficiency = Lateral force due to fins $\rho V^2 A_f$

Thus, a design of cantilever fin was evolved of low drag which was more efficient than the flat fins of R.33 and considerably more so than the cantilever fins of R.38. This illustrates the value of a consistent set of progressive experiments.

It is considered that aerodynamic research on airships has now reached a position where a reasonable and safe technique has been evolved for the estimation of aerodynamic loading under any of the well recognised manœuvres. Further, such evidence as is available tends to show that scale effect is unlikely to disturb the conservative nature of such estimation. The position with regard to the prediction of drag, however, can hardly be said to be on such a satisfactory basis. The principal difficulty in the model experiments is the fact that owing to the large dimensions of the hull as compared with the power cars, control car and other additions, the scale must be so small as to make the accurate representation of these additions almost impossible.[6] The drags of the various additions can, of course, be measured separately on larger models and be added to that for the bare hull alone, but if this is done, due allowance must be made for mutual interference effects, particularly between hull and cars. Therein lies the principal difficulty. Attempts have been made to measure the *static* interference of the cars on the hull and there is a certain amount of evidence that this varies with the longitudinal position of the cars. The dynamic effect (*i.e.*, that of the slipstreams of the propellers on the airflow over the hull) remains unknown at present. It is probably of considerable importance and it is hoped that experiments may be devised for dealing with it in the future—possibly when larger wind tunnels are available. Tests on a composite model usually give a lower figure for the gross drag than that obtained by summing the measured drag of the individual ports plus the effects of mutual interference. Admittedly, the detail cannot be made complete or perfect on the composite model owing to the small scale, but in the case of R.33 the result obtained on the composite model appeared to be nearest to the full-scale determination. There may, of course, be a real scale effect between the tiny model and the huge ship, but the methods of measuring drag on both are hardly accurate enough to enable a value to be assigned to this effect. The full-scale experiments usually show a lower drag than that predicted from the model. The following table shows the predicted drag coefficient of the various parts of R.101 as compared with R.33[7]:—

[6] The scale of the models used in such experiments was:—
For R.33, 1 161. For R.101, 1 217.26.

[7] *Vide* Aeronautical Research Committee's Reports & Memoranda No. 827.

Item.	R.33.	R.101.	Remarks.
Bare hull	0.01150	0.00725	Polygonal cross-section.
Fins and flaps ...	0.00251	0.00152	Includes external bracing wires and control sectors in the case of R.33.
Control car ...	—	0.00015!	The control car of R.33 was combined with the forward engine car.
Engine cars ...	0.00277	0.00081	Four cars for R.33 and five for R.101. Propellers locked for R.33 and rotating for R.101.*
Interference of cars on hull	0.00057	0.00055!	
Radiators	0.00121	0.00038	Items marked ! are estimated only.
Miscellaneous ...	0.00121	0.00090!	
Total ...	0.01977	0.01156	

* The effect of the propeller slipstream is to considerably increase the apparent resistance of the car bodies.

The figures given in the table are in the form of coefficients obtained by dividing the drag of each item in lbs. by $\rho l^2 l^2$ (l^2 = ship's volume $\frac{2}{3}$ = 16,100 for R.33 and 31,000 for R.101).

The model cannot be made truly representative of the full-scale ship in one important respect, *i.e.*, in the exact configuration of the surface of the outer cover. As the cover becomes slack in the process of time, the transverse girders may cause serious ridges in its surface if they are near enough. This has a noticeable effect on drag. It will be appreciated from Fig. 2 that in R.101 the structure has been so arranged with the longitudinals standing outside the transverse frames that there is no fear of this happening.

Structural Arrangement

The general arrangement of the structure of the airship, together with typical sections, is illustrated in Fig. 2.[8]

The following is a summary of the principal features:—

Transverse Arrangement

Cone Suspension to Frame 0.—This is the bow proper with cabin and mooring arrangements (see below).

Frame 1.—This is a flat radially-braced frame arranged after the conventional Zeppelin pattern. It carries at the bottom a hinged platform which can be let down from the ship for the entry of passengers from the mooring tower gallery. The sides of the platform, when lowered, are covered in with canvas. The passengers pass from this platform on to a small landing and from thence to a stairway fixed on the bottom longitudinal girder at the port side of the ship (see Fig. 2). Further aft this stairway passes into a corridor about 2ft. 4½in. wide, which leads to the passenger accommodation. A series of metal arches, covered with fabric, is used to protect the passengers passing along this corridor. It is lit with electric lights spaced approximately 25ft. apart.

Frame 2.—This is a flat radially-braced frame (see Fig. 2) arranged after the conventional Zeppelin pattern, except that the ring is not continuous, the transverse girders being hinged to one another. There is a platform at the bottom of this frame from which the main trail rope and two standing guys can be lowered through hatches. The hatches are released by distant control from the control cabin.

[8] See also Journal Royal Aeronautical Society, Vol. XXXIII., No. 219, p. 177. There are several errors in this drawing, however.

Frame 3.—This is a novel type of "ring" frame without radial bracing (see below). Frames 3 to 13 inclusive are all of this type.

Frame 4.—This frame carries two power cars.

Frame 5.—Access to a cockpit in the top of the ship can be obtained by means of stairways passing round inside this frame.

Frames 6, 7 *and* 8.—These frames carry the passenger accommodation (see below).

Frame 9.—This frame carries two power cars.

Frame 10.

Frame 11.—This frame carries a single power car on the centre line.

Frames 12 *and* 13.—Between these two frames the cross-section changes from a polygon of 30 sides to one of 32 sides. For this purpose the top longitudinal girder is forked into two branches at its aft end. Other longitudinal girders are slightly skewed. The reason for this change in the cross-section is to simplify the problem of fitting in the fin structures.

Frames 14 *and* 15.—These frames are of special construction in order to carry the fin structures.

Aft of Frame 15 *to Tail Cockpit.*—This is a light conical structure of conventional Zeppelin type girder work (see Fig. 3).

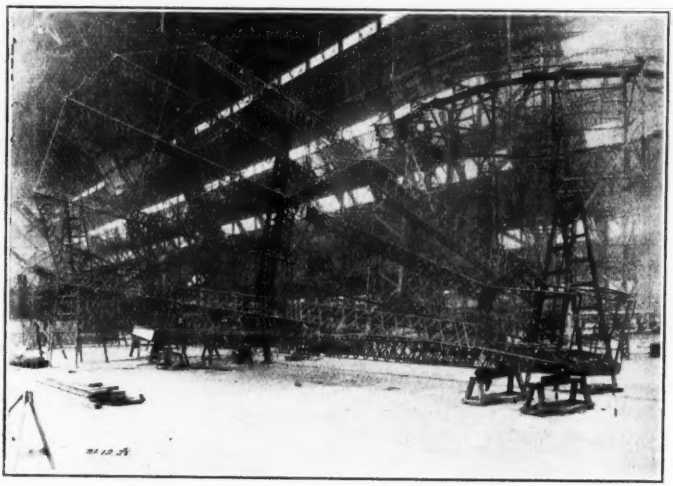


FIG. 3.
Tail structure.

Longitudinal Arrangement

There are 15 main longitudinal girders. It will be seen from Fig. 2 that they are located entirely on the outside of the transverse frames and that the span between the two bottom girders is less than the other 14 sides of the cross-section. The reason for this is economy in the span of the platforms, decks, etc., which are joined to these girders in various places. There are 15 intermediate longitudinal girders, the use of which is merely to assist the main longitudinals in carrying the lateral loading caused by the outer cover. They also reduce the tensions in the outer cover fabric itself. In short fat shapes, such as that of R. 101, if all the transverse frames were equally spaced, the volume of a gasbag towards the centre of the ship would represent too large a proportion

of the total. It will be seen from Fig. 2, therefore, that the gasbags are made progressively longer as they pass from the centre of the ship towards the ends. There are 16 gasbags including one in the tail cap behind the fins. The two foremost bags are inter-connected and so are the two aftermost ones. This is for the purpose of economy in the number of valves required.

Ring Frames Without Radial Bracing

These frames, together with the special type of gasbag wiring which they require, constitute the principal structural novelty. A full description, together with the reasons for their adoption, has already been published by the author, and space does not permit of repetition here.[9] It has been suggested that a serious objection to this type of frame lies in the fact that it causes a loss of lift equal to the internal volume which it occupies. In R.101 the frames of this type occupy a total volume of 130,000 cu. ft., equivalent to a lift of approximately 4 tons. This hardly appears a fair criticism, for let it be supposed that the gas volume *is fixed to give the required lift* then the *outer cover volume* will need to be 130,000 cu. ft. more in a ship with the new type of frame than in one with the old type of frame, and it is the effect of *this* difference which should be examined. On a conservative estimate it is calculated that this increase in outer cover volume would necessitate an addition of 1/4 ton to the fuel and machinery weights in order to maintain the same

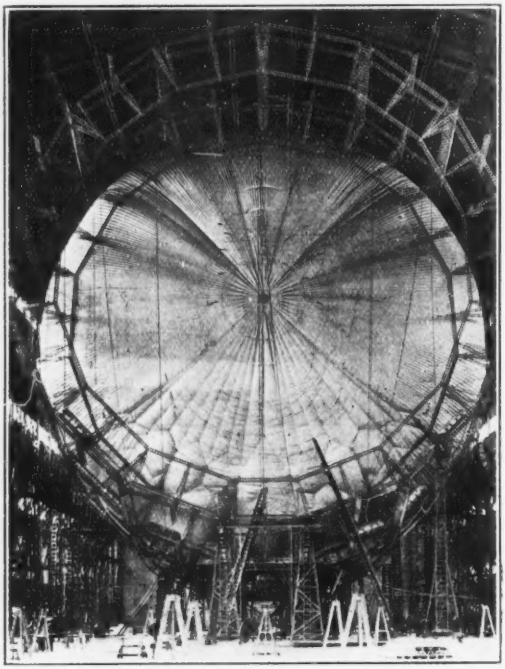
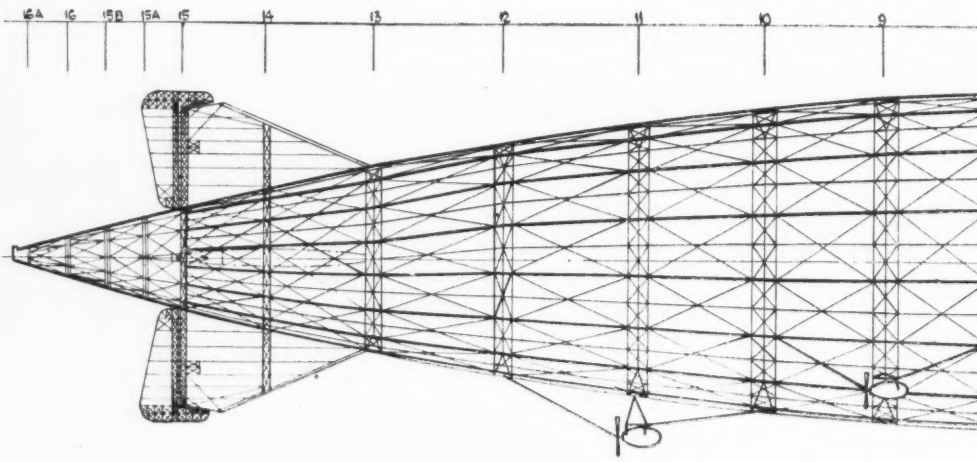


FIG. 4.

Trial inflation of one of the largest gasbags.

[9] Inst. of Naval Architects *loc. cit.*



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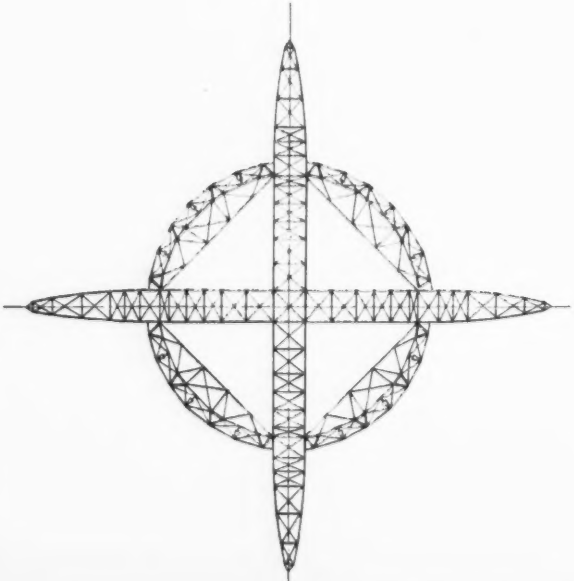

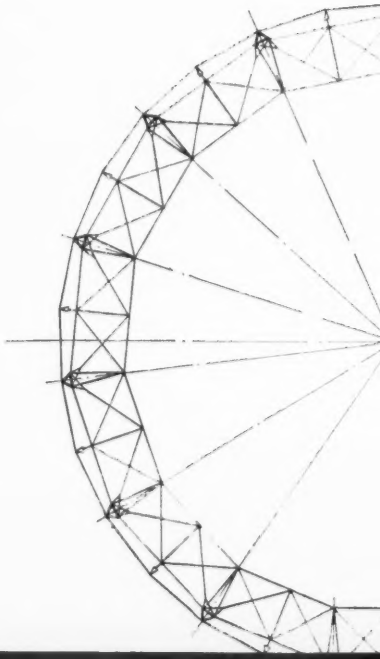

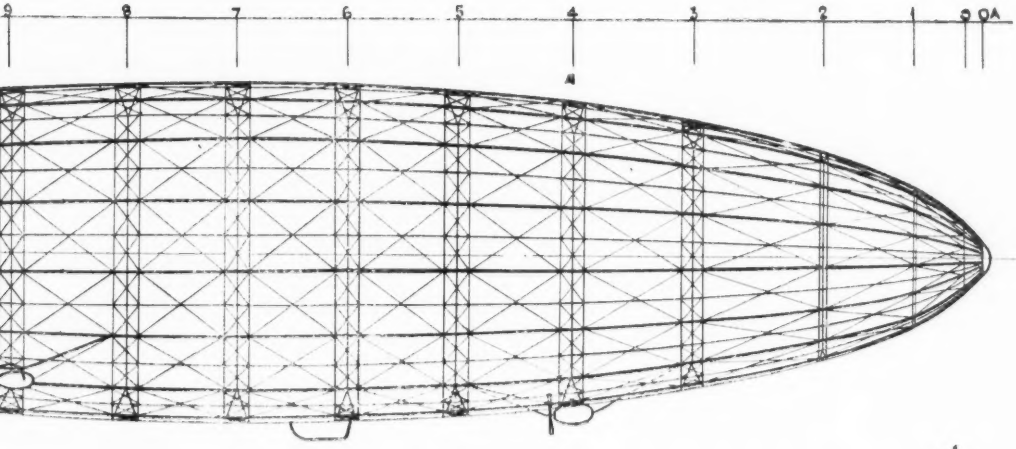

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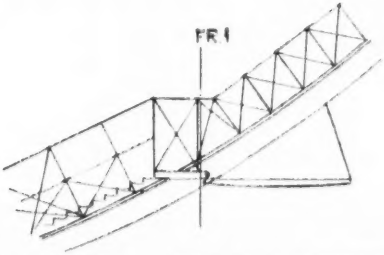
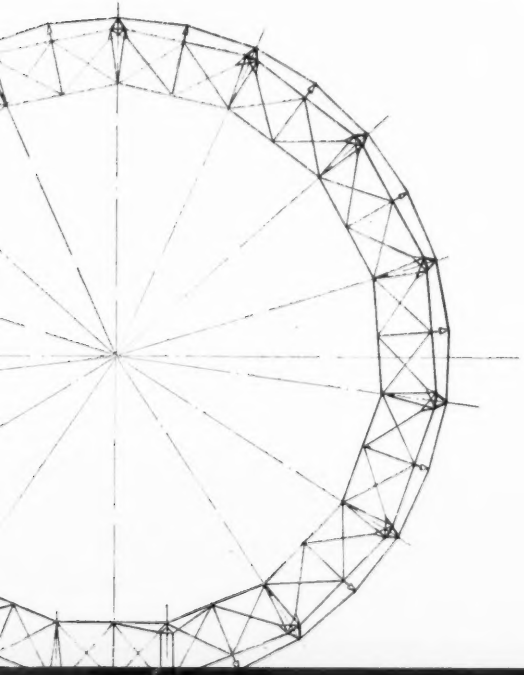
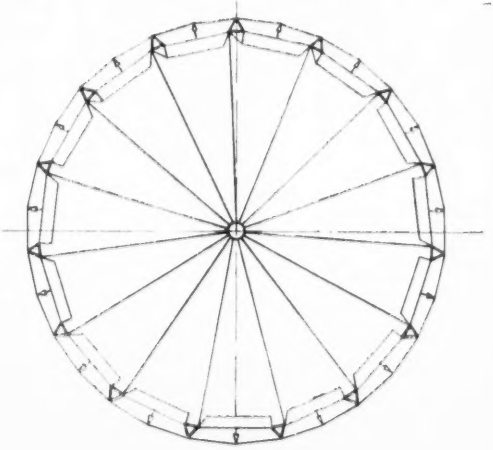
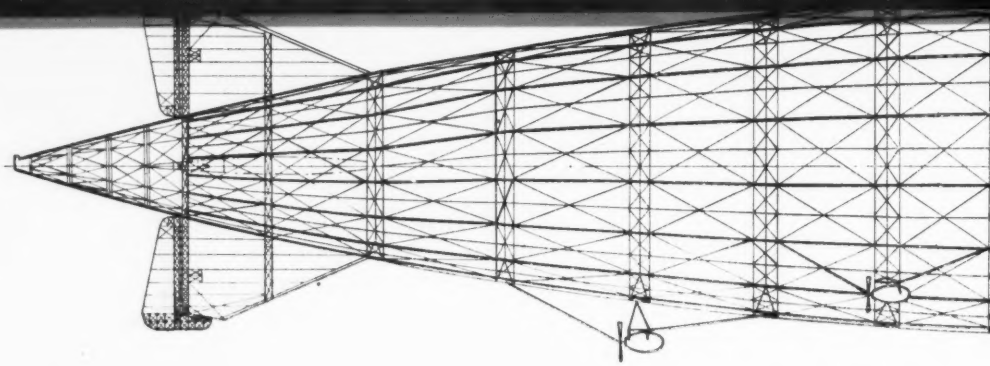


VIEW SHEWING ACCESS PLATFORM







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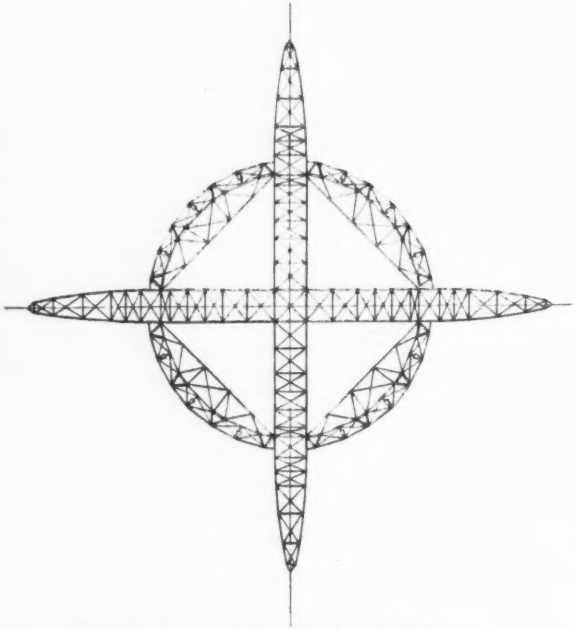
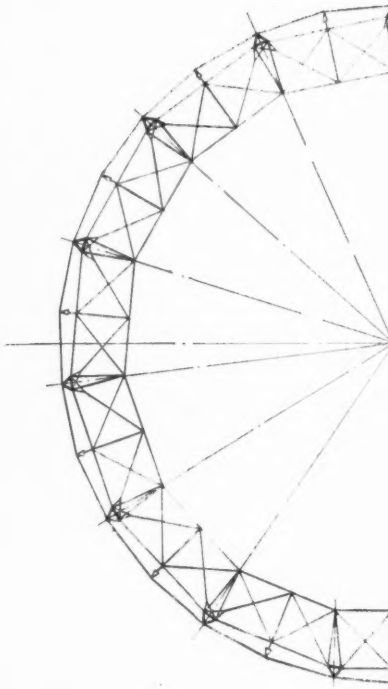


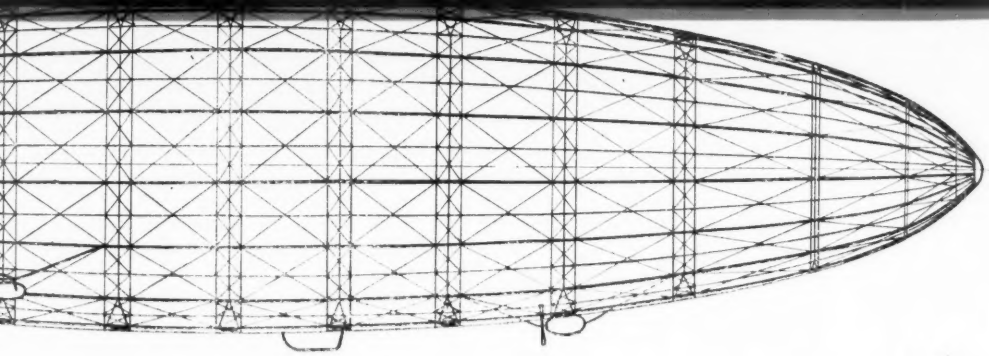
—SECTION AT FRAME 14—



—TYPICAL
FRAME

FIG.

General structural arr



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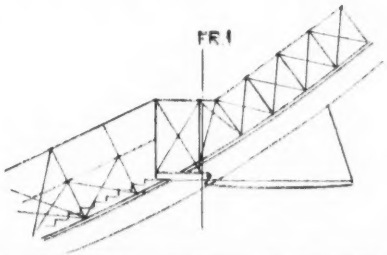

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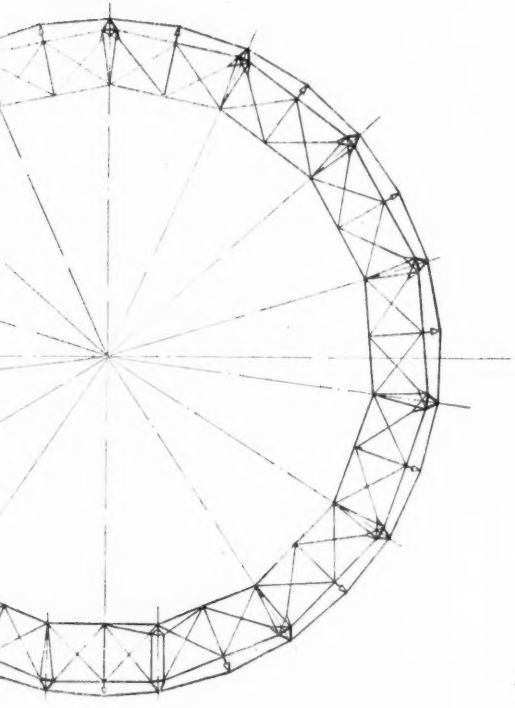
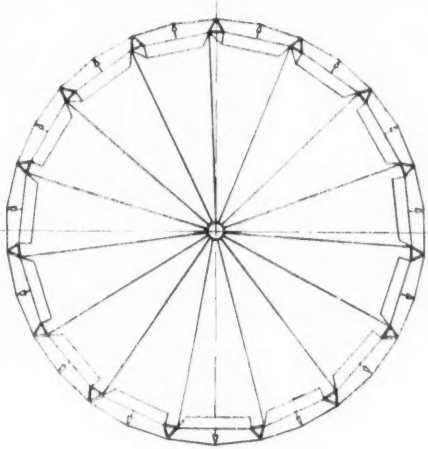




—SECTION AT FRAME 2—

PICAL SECTION—
FRAMES 3 - 13

FIG. 2.

ctural arrangement of R.101.

speed on a non-stop flight of 2,500 miles. Furthermore, there would be an increase in the weight of the outer cover and structure of about half ton since this is slightly larger. The net effect should be regarded, therefore, as adding three-quarter ton weight to the airship rather than subtracting 4 tons from the gross lift.

It must be remembered that these frames provide valuable stowage for the fuel and ballast system, miscellaneous platforms, ladders, and passenger accommodation. They permit of these items being fixed in place while the frames are still lying flat on the ground and thus avoid a large amount of fitting work having to be carried out at a considerable height. Any alternative stowage, with the old type of frame, as for example, in a keel, would mean a definite subtraction of volume, and there is certainly no reason for believing that ships of 5 million cu. ft. and over would prove any lighter with the old structural arrangement, in fact, the reverse is probably true. A similar view appears to be held in America, where it is understood the designers intend to adopt the new type of frame for the two 6½ million cubic ships just ordered from the Goodyear Zeppelin Corporation by the United States Navy Department.

By far the largest internal air volume is the annular space between the gasbags and the outer cover, amounting to approximately 455,000 cu. ft. when the gasbags are full. Some such space is necessary for ventilation, access to the structure, and to prevent the outer cover approaching near enough to the transverse frames to form undesirable ridges. The problem of determining the lengths of the wires of the gasbag netting system is very intricate. It was felt desirable, until experience had been gained, to be quite sure that ample clearance from the girders had been allowed. On the inflation tests which have been carried out it has been found that the gasbag has taken up a shape agreeing very closely with what was predicted. It should be possible in future, therefore, to design the gasbag wiring system much closer to the girders, thus reducing the annular air space and probably increasing the gross lift from 151 tons to 155 tons. Fig. 4 shows a trial inflation of one of the gasbags of the airship. The special system of gasbag net employed is clearly shown.

The Bow

The theoretical shape for the bow of the ship is bluff and rounded. The adaptation of this to carrying a heavily loaded mooring spindle introduces certain structural and clearance problems. The spindle must be supported in a pyramid of members. In order for this pyramid to clear the gauge line of the mast head there must be a certain overhang of the spindle, or else the vertical suspension of the cone must be considerable. These two alternatives are illustrated in Fig. 5. A fair overhang on the spindle implies a fair weight to resist the bending and a big reaction on the forward bearings. A deep vertical suspension to the cone means a large reaction where the ship's cone is locked to the mast cone, and a short overhang to the spindle involves certain difficulties in fitting the pyramid of members which carries it neatly to the theoretical bow shape. On the whole the former alternative is preferred in R.101. The mast reaction causes a heavy shear in the forward portion of the hull. In the bay immediately behind the nose pyramid it is very difficult to arrange effective shear bracing. Consequently the longitudinal members are made to aim directly at the suspension point so that they may resist the shear by end load alone. The method of adapting these members as a tangent cone to the theoretical bow shape in R.101 is shown in Fig. 6. The front face of the first gasbag is some 8½ ft. back from the mooring spindle, and a platform is slung in the intervening space, thus forming a small cabin for the stowage of the three winches which carry the mooring rope and side guys. Handy connections are also arranged in this space for a supply of gas, fuel, water, compressed air and electricity from the mooring mast. A small crows nest

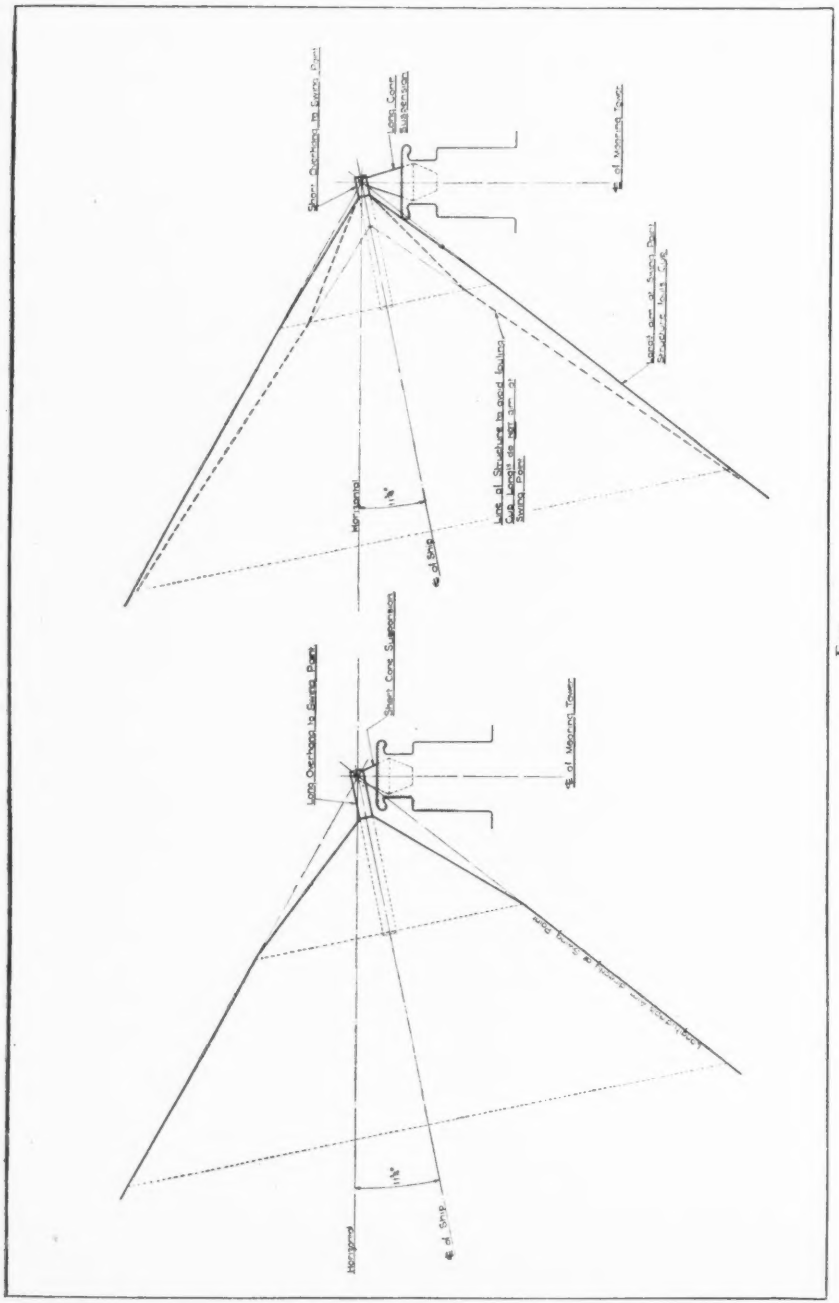


FIG. 5.
Figure illustrating the methods of providing clearance at the bow.

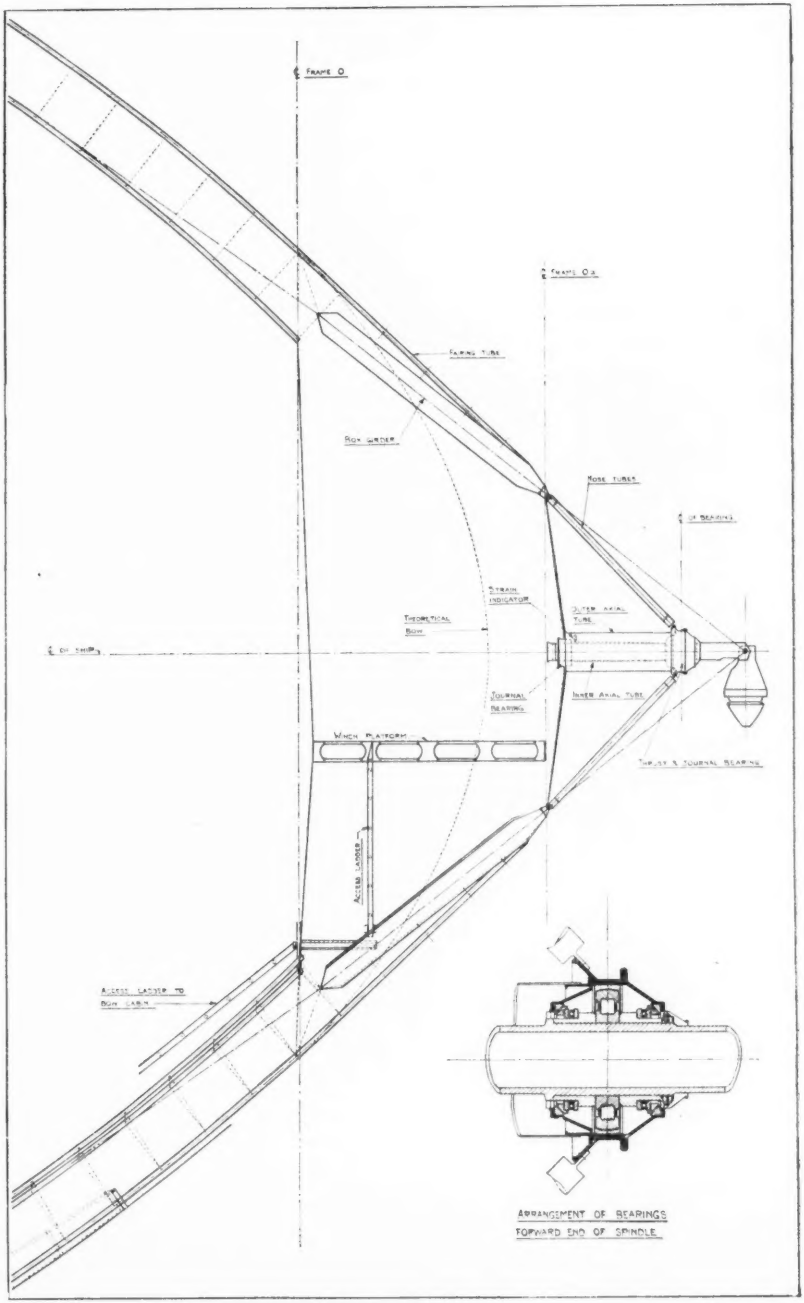


FIG. 6.
General arrangement of the bow.

FIG. 5.
Figure illustrating the methods of providing clearance at the bow.

on top of the bow is reached from this platform. There are windows on either side and a hatch below for letting out a drogue. Great care must be taken to design the mounting of the spindle so that the bearings cannot be jammed by its distortion under bending load.[10] A journal bearing with thrust bearings on either side is mounted in a housing on the forward end of the spindle. Outside these bearings spherical seatings are provided so that the whole assemblage of three bearings can move, as it were, on the surface of a large sphere if the spindle distorts (see Fig. 6). Special instruments are provided whereby the bending of the spindle is utilised to measure the reaction at the mast head. The general arrangement of the structural members in the bow is illustrated in Fig. 7.

The Fin Structure

Large cruciform girders are built into frames 14 and 15 to carry the

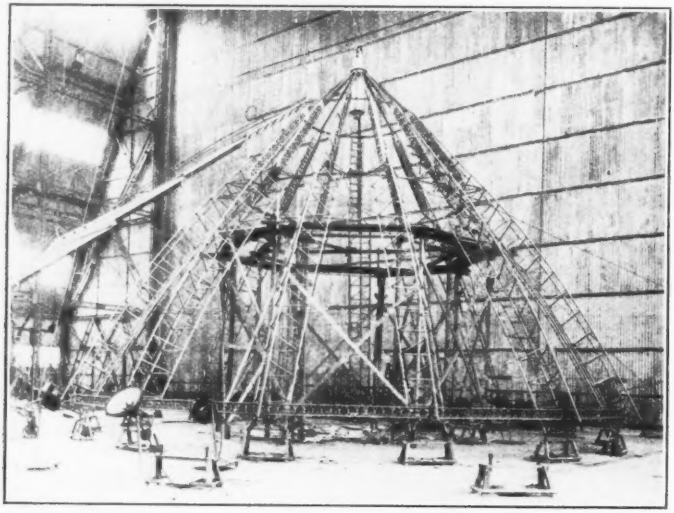


FIG. 7.
Bow structure.

fins. The frames themselves consist of four trusses supporting these girders (see Fig. 2). Cantilever posts are attached outside the ship to these cruciforms, thus forming two spars for the support of each fin. The tops of these spars are inter-connected to one another and to frame 13 by means of a strong leading edge girder. A series of longitudinal ribs interconnect the two spars and the leading edge girder and serve to give the correct longitudinal profile to the fin.

The flaps are operated by an ingenious device, the design of which (and also of several other important features in the ship) is due to Squadron Leader F. M. Rope. The first object is to do away with the external sectors, king-posts, cables, etc., which are usually a serious addition to the drag of the ship. *AB*, *AC* (see Fig. 8) are two tubes connected to the flap forming a lever which projects into the interior of the fin ahead of the flap hinge. The

[10] Failure to provide for this is believed to have caused the American airship Shenandoah to break away from her mooring mast.

connection of *B* and *C* to the flap is by means of bearings forming a hinge inclined to the vertical hinge line of the flap. The end *A* of the lever is provided with a roller which runs in vertical guides fixed in the fin. When *A* travels vertically above or below its mean position the flap moves to port or starboard of its mean position. Cables running over pulleys connect the ends *A* of the four fins to the Servo mechanism, which is located in the bottom fin. Combined with this mechanism is the auxiliary steering control which will function if the control car or cables passing through the ship are damaged.

The aerodynamic model experiments on rudder balance showed that owing to the interference of the hull with the air flow over the fins, it was not practicable to obtain a high degree of balance with a simple form of horn, without introducing objectionable characteristics under certain conditions. The experiments were extremely difficult because they involved the measurement, on a very small scale, of hinge moments which should be small on the full scale. For the same reason deductions from them can only be accepted with extreme caution. It was decided, therefore, to design the structure of the horn so that

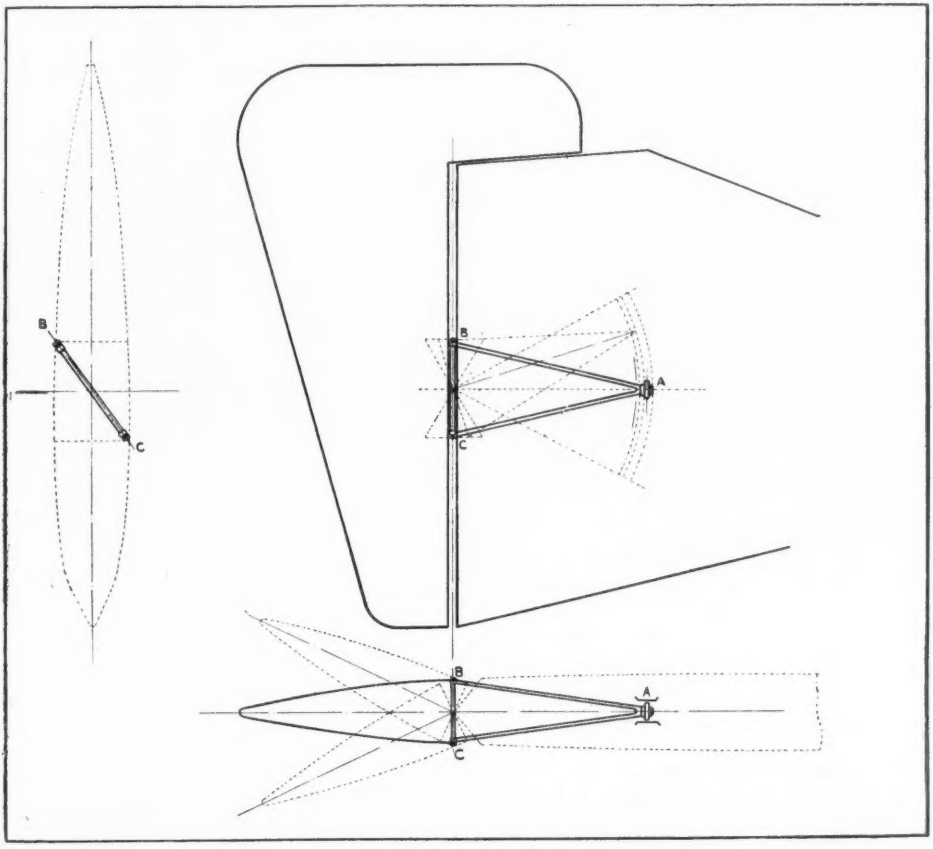


FIG. 8.
Flap control lever.

it could be easily replaced by one of different dimensions if the full scale trials of the ship showed this to be desirable. It was also decided to provide power assistance for the operation of the flaps, though this can equally be simply eliminated should the full scale trials show it to be unnecessary.

The power gear consists of one 3 h.p. electric motor, serving both rudders and elevators through hydraulic variable speed gears. Mechanism is arranged whereby the proportion of work done by the helmsman increases with the rate at which he turns his helm. This acts as a check on unnecessarily violent operation of the flaps, and also implies gentle movement of the rudders even when the resistance to movement is small, if the effort applied to the wheel is not too violent. In case of failure from any cause whatever, the power gear automatically cuts out without action on the part of the coxswain, who then can continue to operate the flaps, though he has the whole of the work to do at the same gear ratio as before. The high ratio enables him to move the controls rapidly through large angles when the air speed is low (as, for example, when manœuvring for landing on the ground or to the mooring mast) although it may involve reducing the speed of the ship when flying except in a very steady atmosphere.

Passenger Accommodation

The passenger accommodation is located on two decks placed entirely within the contour of the hull. These decks are spread over two bays of the structure, approximately under the centre of buoyancy.

A good deal has been said regarding the standard of comfort which it is reasonable to expect in an airship. This must to a certain extent depend on the length of the journey to be flown between successive stopping places and hence the time occupied, and it is the opinion of the author that only future experience can decide the maximum comfort which it is reasonable to provide on a given journey for a reasonable structure weight. Non-stop journeys of $2\frac{1}{2}$-3 days were visualised for R.101.

A compromise must be struck between the claims of space and provision of sufficient daylight and outlook for the passengers. If, in addition to sleeping cabins, it is considered necessary that separate rooms should be provided for general recreation, for feeding, and for smoking, then the floor area required is considerable. A passenger car located entirely outside the hull (such as has been used in all German passenger carrying airships and in the British airship R.36) cannot be made very wide for structural reasons, and also because of the large increase in resistance which would be entailed. If it is to have floor area of the extent referred to above, it must perforce be very long. This leads to various disadvantages. The added resistance is considerable, the proportions of the living rooms are long and narrow, somewhat like a train, thus giving a cramped feeling, and any general movement of the passengers from one end of the car to the other considerably disturbs the trim of the airship owing to the length of the car. Such a car has the advantage, however, of providing more daylight and a greater window area for the outlook of the passengers.

Some advantage has also been claimed for the fact that the windows are more vertical than is possible with an internal passenger car and they therefore permit the passengers to lean out of the windows. It seems doubtful, however, whether it would be possible to have the windows open for any considerable length of time with airships travelling at the present contemplated speeds. It would seem, moreover, that a window sloping at approximately 45° would provide a more interesting outlook than a purely vertical one.

The internal arrangement allows of saloons having a much better proportion of width to length, and avoids any undue disturbance of the trim owing to the more restricted longitudinal movement of the passengers. It

has been argued that the internal passenger car means a definite sacrifice in the gas volume of the airship. In R.101 this amounts to approximately 100,000 cu. ft., which is equivalent to a lift of 3 tons. It must be clearly realised, however, that had a car of the same volume been put outside the airship, even if its dimensions had been designed to give the minimum possible resistance, it would have added approximately 12 per cent. to the gross drag of the ship. It is calculated that, in order that the speed should not be reduced, an additional engine and fuel weight of approximately 4 tons would be needed on a non-stop journey between England and Egypt.

The living rooms in R.101 do certainly avoid the cramped feeling which is experienced in the long narrow train-like external car. Ample outlook is provided from windows sloping at 45°, located along the promenades at the sides of the main living rooms.

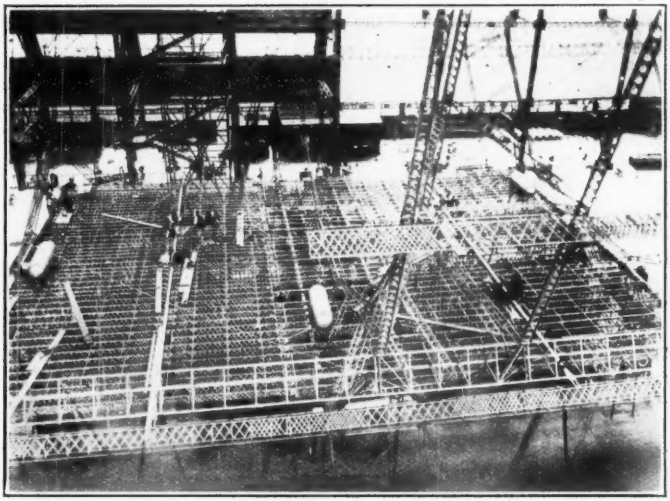


FIG. 9.
Girder work of upper deck.

The structure weight involved in the passenger accommodation of R.101 works out at approximately 225 lbs. per passenger. Doubtless this could be improved upon slightly in further designs without sacrificing the degree of comfort.

The question of roll has never proved troublesome in past airships; in fact, it has been almost imperceptible. It has been argued, however, that placing the passenger accommodation within the hull raises the centre of gravity, a state of affairs which has also been aggravated by the fact that the fuel tanks in R.101 have not been placed as low relatively as in previous airships and that excessive rolling will occur in consequence.

Had this airship been built to the fineness ratio previously employed, there might be something in this contention, though the author is of the opinion that this is a point which could only be decided by actual experience. The fact, however, that the centre of gravity has been considerably lowered by the reduction of the fineness ratio, amply disposes of this contention.

The upper deck (the girder work of which is shown in Fig. 9) has an area, exclusive of the promenades, of 5,550 sq. ft. The lower deck has an area

of 1,750 sq. ft., which can be increased if found desirable to approximately 4,000 sq. ft., by means of side wing decks.

The average weight of the girder work for the decks is 1.2 lbs. per sq. ft. The covering consists principally of birch three-ply, strengthened below with ribbing of spindled spruce. The average weight of this covering is 0.8 lbs. per sq. ft. The result of several experiments was to show that the most economical result was obtained with closely spaced girders and a thin covering, rather than *vice versa*. The open portions of the deck, which are free from obstruction, are made to withstand a uniform dead loading of 170 lbs. per sq. ft. Experimental sections were tested with men packed as closely as possible jumping or marking time. The chief problem in the design of the decking was, however, the avoidance of unpleasant deflection rather than the provision of adequate strength. For the floor of the smoking room a $\frac{3}{4}$in. thickness of Balsa wood has been employed, sheets of indented aluminium being glued to the upper face and a veneer of poplar being glued to the under face. The edges are also sealed. This makes a very stiff covering, and weighs 1.1 lbs. per sq. ft. The covering is cut away under settees, bunks, etc., and the space is filled with fabric in order to save weight. The support of these decks within the hull framework proved a by no means easy problem. It is essential, of course, that any distortion, either of the hull framework or the decks, shall not produce an indefinite partition of stress. The method of overcoming this difficulty is diagrammatically illustrated in Figs. 10 and 11.

Midway between consecutive transverse frames a structure similar to an inverted roof truss and referred to as a " transverse half frame " is suspended to provide intermediate support to the fore and aft floor bearers. This can rise or fall according to the strain in its suspension wires, independently of the main hull framework. At the main transverse frames the other principal transverse supporting members are located with hinges at their suspension joints (see Fig. 11). Apart from this the figures will be found self-explanatory.

The top deck carries the principal saloon or lounge, 60 ft. by 32 ft., flanked at the sides with promenades 7 ft. 6 in. wide. From the promenades an outlook for the passengers is provided through windows sloping at approximately 45°. The lower portion of these windows is of Triplex glass carried in a strong girder, which also carries a rail on which the passengers can lean. The portion of the windows above this is of Cellon.

The upper deck carries a dining room, 32 ft. by 20 ft., to which daylight is admitted through Cellon windows. A small food lift connects the dining room with the kitchen which is located immediately below it on the lower deck.

This deck also carries sleeping accommodation for 52 passengers located in two berth cabins, the floor area of each cabin being approximately 6 ft. 6 in. by 5 ft. 3 in. Wash basins for ladies and gentlemen are also located on this deck. These consist of hand basins arranged in separate cubicles, with a supply of hot and cold water.

The lower deck carries a small smoking room, 16 ft. by 12 ft. 9 in., lavatories, and crew's living room. The crew's sleeping bunks are slung on either side of their central living room and further sleeping accommodation for passengers can also be arranged on wing decks at this level slung on either side of the central portion.

The captain's control room is located in the forward part of this deck, with open access to the main control car, which is slung outside the hull of the airship immediately below. The control car is placed approximately at the centre of the ship in order to provide a good view of the ship both forward and aft. Adjoining the control room is the wireless cabin and the kitchen.

Some of the principal controls are duplicated so that they can be operated either from the control car or alternatively from the control room above.

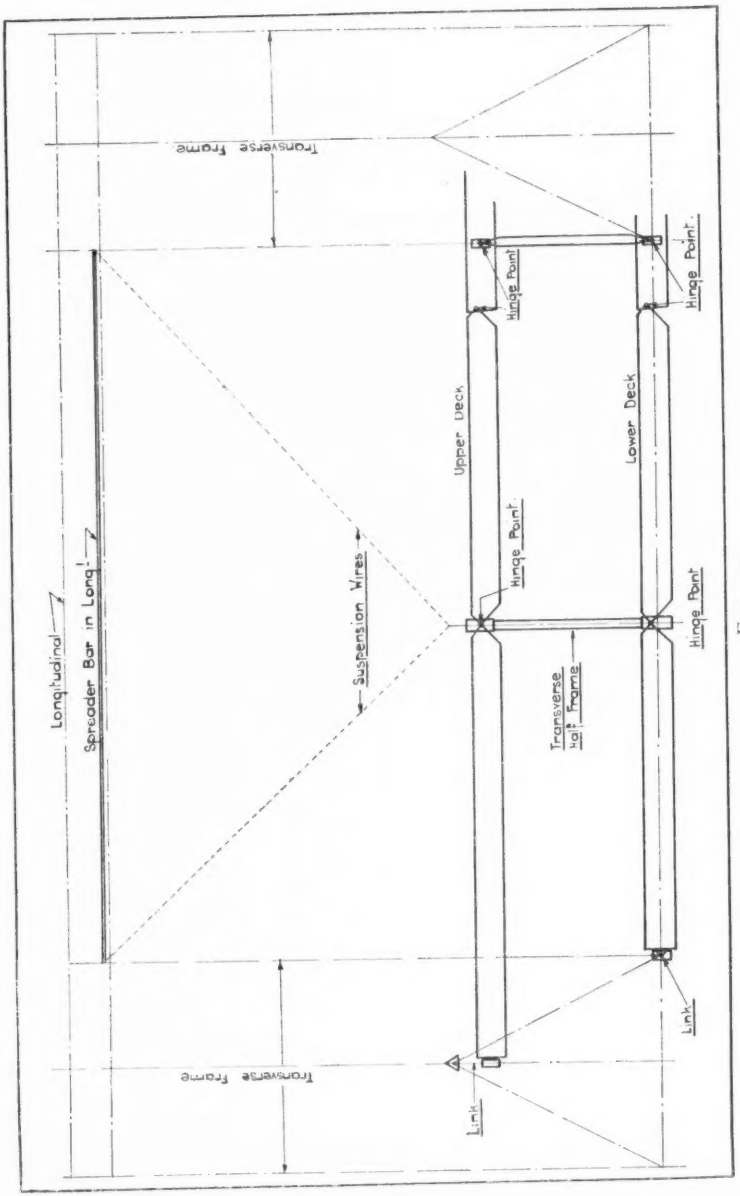


FIG. 10.
Slinging of passenger decks (longitudinal arrangement).

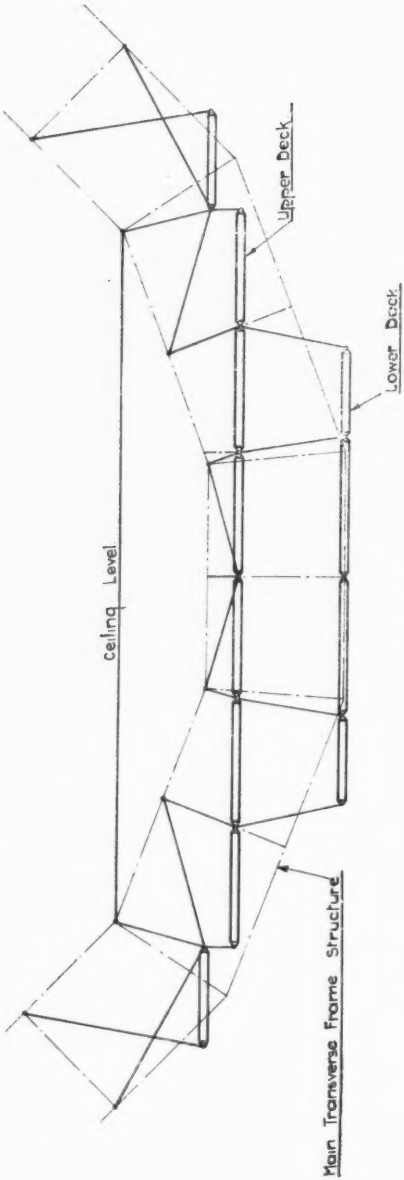


FIG. 11.

Slinging of passenger decks (transverse arrangement).

The walls of the principal living rooms consist of light duralumin girder work across which fabric is stretched. The fabric is further supported and decorative effect is obtained by means of half columns, mouldings and shelves of balsa wood. It was found quite feasible to work decorative mouldings and columns of this material. A typical view of such a bulkhead is shown in Fig. 12. In general settees, shelves, etc. are arranged round the walls so that passengers are prevented from contact with the fabric portions. This type of wall works out at approximately .4 lbs. per sq. ft., including vertical girder work, balsa mouldings, fabric, dope and paint, but exclusive of settees.

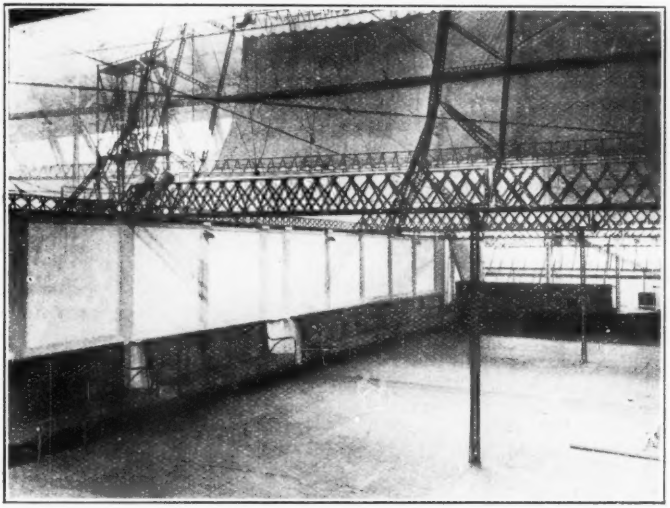


FIG. 12.

Typical bulkhead in passenger accommodation.

STRUCTURAL DETAIL

The general structural arrangement having been decided upon, it was considered advantageous to make certain radical departures from the principles which had hitherto governed the design of the details of the hull structure of previous rigid airships. Apart altogether from the fact that increase in size and speed necessitated the design of new girders to meet the increased span and loading, it was felt desirable in any new design to make provision for simplifying and speeding up the process of erection, and subsequent repair or replacement of members. It was obvious from the outset that improvement in the desired direction could only be achieved by a radically new design of joint. Incidentally this is desirable for other reasons, since the old type of rivetted joint, with its multitudinous gusset plates, gives a very indefinite distribution of stress and tends to rack loose in the course of time. The ideal aimed at was a joint which would enable the various girders to be made accurately to length beforehand, and then to be joined together on erection with accurately fitting bolts. Apart altogether from the very arduous work of stress calculations for the thousands of hull girders, the task of determining their dimensions to the necessary degree of accuracy for them to be jigged up to length before erection, was a very formidable one.

In order to allow the structure to be put together with accurately fitting bolts it was considered necessary to work to tolerances ranging from 0.015 in. on girders approximately 11 ft. long to 0.030 in. on girders approximately 45 ft. long. In some cases this necessitated calculating the geometry of 15 significant figures which precluded the use of slide rules, etc.

The girder work which forms the shell of the airship has to carry attachments for a very wide variety of items, such as :—

Gasbag Wiring.
Fuel and ballast, tanks and piping.
Power cars.
Control car.
Passenger accommodation.
Nose spindle and tail cap.
Outer cover support girders.
Miscellaneous gangways, ladders and platforms.
Handling and mooring guys.
Controls.
Valves, etc.

If the consideration of these items is left until after the design of the main shell has been completed the provision of the necessary attachments may well delay erection very considerably, besides complicating the equilibrium conditions at the joints, thus leading to undesirable secondary stresses. It was thus thought essential, as part of the ideal scheme of design aimed at, to consider side by side with the determination of the geometry and loading of the main girder work, the question of general arrangement and design of all the items specified above. This entailed fixing the position and loading of the thousands of attachments required. It may be argued that such a process means a vast amount of work in the drawing office before construction can be commenced. It has also been objected that the type of joint indicated above necessitates the manufacture of a vast number of machined parts.

It is the opinion of the author that, leaving aside the time required in investigation and experiment on the first airship of such an unorthodox type, the policy pursued will eventually lead to a definite saving of both time and cost. Machining work should prove cheaper than the hand labour involved in cutting gusset plates, etc., and there is no doubt, from experience already gained, that erection, with all the difficulties of working above the ground, has been speeded up and simplified. Very simple cradles suffice, and in spite of the novelty of the problem erection in the shed has proceeded faster than the components could be supplied. The position twelve months ago was that only three rings had been erected and three others were on the floor in various stages of completion. To-day the ship is within two months of absolute completion.

Not only is a reduction in the heavy labour bill for erection made possible, but it must be remembered that the capital locked up in sheds for construction is so large that it will be important, when airships are required in numbers, to get each one through its constructional berth as quickly as possible.

Similarly, owing to the relatively high capital cost of the airship, it is obviously desirable to develop a structure which will have a long life and which will be cheap and easy to maintain and repair. It has been suggested that, owing to the fact that rigid airship design is liable to undergo a good deal of modification in the future, before it becomes in any way standardised, it is not necessary or even desirable at this stage to endeavour to produce a hull which will have a long life if the capital cost can be reduced by a less permanent structure. Such an argument would seem, however, to lead to a vicious circle, since it indefinitely postpones the process of the development of the design of a permanent form of structure.

The design, geometrical and stressing work outlined above having been accomplished, there remained the considerable task of the design and subsequent manufacture of the structural details of the shell of the airship to meet the ideals aimed at. After some preliminary experimental members had been produced the whole of this work was placed in the hands of Messrs. Boulton & Paul, of Norwich. The highest possible credit is due to Mr. J. D. North, Director and Chief Engineer of the firm. Mr. North acted as consultant to the Director of Airship Development in this work, and was thus able to bring his extensive knowledge of metal aeroplane construction to bear on the improvement of the design of the structural details of the airship. Very great credit is also due to Messrs. Boulton & Paul for the excellence of their workmanship in the production of the thousands of components.

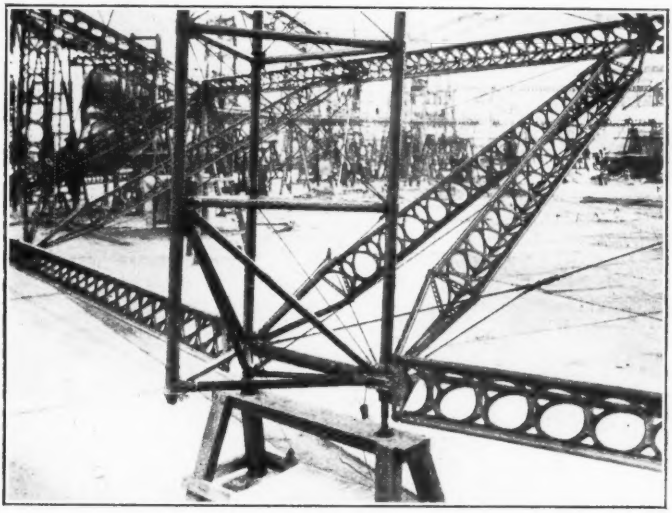


FIG. 13.
Structural detail.

It is not possible within the scope of the present paper to give an adequate account of the structural details, but very full descriptions illustrated by numerous photographs have been published in the Technical Press, and a reference to these will be found below.[11]

Examples of the structural detail are shown in Figs. 13, 14 and 15. Fig. 13 shows one of the main transverse frames lying horizontally in the process of erection. This figure shows radial struts, ridge girders, and a portion of a longitudinal girder. These are the three principal types employed in the hull framework. Figs. 14 and 15 show typical joints.

Steel has been used for about 25 per cent. of the total weight of the girders. It has been employed wherever suitable for the sole purpose of saving

[11] *Flight*, November 29th, 1928, p. 1020.
Aeroplane, November 29th, 1928, p. 881.
Engineering, November 30th, 1928, p. 689.
The Engineer, November 30th, 1928, p. 598.
The Engineer, December 7th, 1928, p. 622.
"Some Modern Developments in Rigid Airship Construction," V. C. Richmond, Inst. of Naval Architects Spring Meeting, March 30th, 1928.

weight, and thus improving the efficiency of the girders. Greater efficiency has also been obtained by the use of tubular booms in the place of the open channel sections previously employed. At the same time these tubular members introduce special difficulties in the construction of suitable joints. The figures serve to illustrate the method by which these difficulties have been overcome.

The production of these new types of girders and novel arrangement of the structure has involved an immense amount of mechanical testing both on components and on assembled portions of the structure.

To give an adequate account of this work would require a lecture to itself. Approximately one hundred girders have been tested to destruction, and the essential part of the work has been carried out by the Mechanical Testing Department of the Royal Aircraft Establishment. The fullest acknowledgment is due to the Staff of that Department for their valuable help.

In addition to the testing of individual components a complete full-sized trial bay of the airship was constructed and tested, being fixed to the door

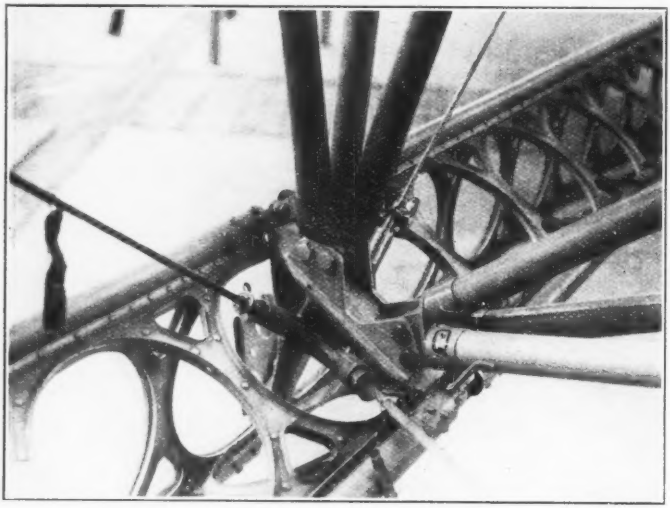


FIG. 14.
Typical joint.

of the shed for this purpose. During the loading of this structure the strains in the various components were measured by means of special electrically recording strain gauges constructed by the Cambridge Scientific Instrument Co. These instruments have already been described.[12] A brief account of some of the tests carried out has been published.[13] These tests were of a very extensive nature, but space does not permit of a description here.

There are many items in the construction of the ship which cannot be dealt with for a similar reason. A full description of important work carried out by Wing Commander T. R. Cave-Browne-Cave and others in the development of the power plant and fuel installation has already been given before this Society.[11]

[12] *Vide* Journal of the Royal Aeronautical Society, Vol. XXX., No. 190, p. 565.
[13] Inst. of Naval Architects *loc. cit.*
[11] *Vide* Journal of the Royal Aeronautical Society, Vol. XXXIII., No. 219.

There are also many fabric details of interest. The outer cover may be cited as one important example in which a departure has been made from previous practice. In order to avoid the objections of doping this huge expanse of fabric in place, the material has been mechanically spread with a special dope beforehand and is tautened on the ship by mechanical devices. This has meant the development of a special form of lacing edge, with lacing points opposite each of the transverse bracing struts of the longitudinal girders. In attaching these points a special tool is used which is illustrated in Fig. 16. By means of a screw device the tension on each lacing point can be given a definite value, which is shown on a spring indicator. Should the cover become slack in the process of time, it can be re-tautened to a certain extent by forcing outwards the intermediate girders referred to earlier, which are attached to the main hull structure by means of adjustable telescopic kingposts.

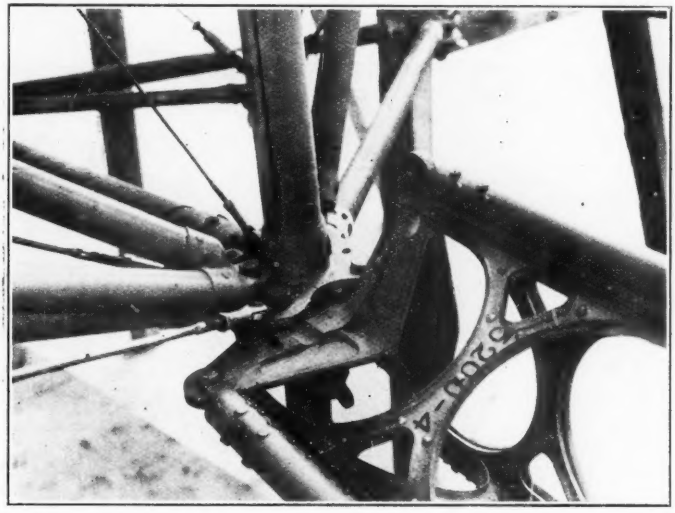


FIG. 15.
Typical joint.

PRESSURE CONTROL

The question of the control of the pressure difference between the interior and exterior of the gasbags and of the outer cover of a rigid airship is one of considerable importance, and appears in the past to have hardly received the attention it deserves. A study of the patent literature on the subject tends to show that various inventors have either missed some essential point in the problem or else have suggested solutions which are hardly likely to prove feasible in practice.

It is, of course, essential to settle at the outset the maximum rate of rise or fall for which the airship must be made safe, and the accident to the airship Shenandoah in America has served to emphasise (if any emphasis was necessary) the importance of this point. Considerable meteorological evidence has been gained on the maximum vertical speed of air currents, principally from a study of the formation of hail-stones in the heart of a thunderstorm. As a result of this it was decided to endeavour, in the design of R.101, to deal

with vertical currents rising at the rate of 4,000 ft. a minute (45.5 miles per hour) and falling currents of approximately half this speed. It is estimated that this rate of rise would correspond to conditions which would produce hail-stones of approximately $\frac{3}{4}$ in. in diameter.[15] More rapid vertical currents than this would be necessary to lift the roofs of buildings, and in this respect airships are not yet "as safe as houses." The same might be said of ocean-going liners, but the risk of being unable to avoid such a rare occurrence must be so small as to be almost negligible.

Rates of vertical movement of the order stated call for very rapid rates of discharge of air and gas, and thus introduce the special problems in the design of gas valves and air vents for the outer cover.

A study of design of past rigid airships, so far as the evidence is available, goes to show that the maximum possible rate of discharge of the gas has only been good enough for approximately a quarter of the vertical speeds mentioned above, and the rate of discharge of air has hardly received any attention at all, except possibly in the last one or two German airships.

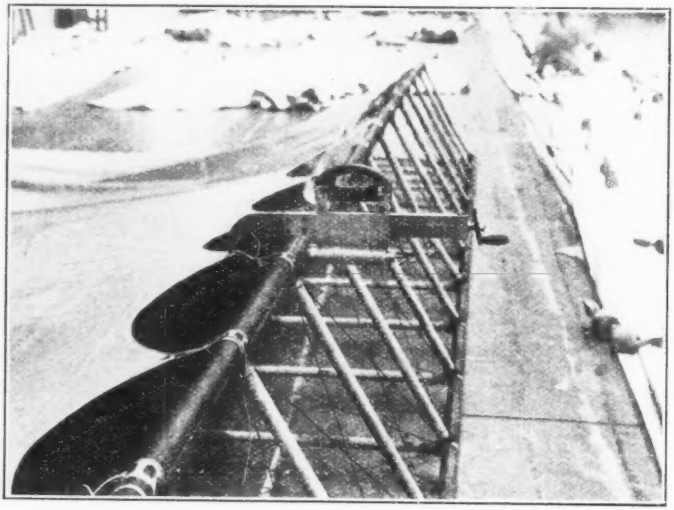


FIG. 16.
Adjusting tension in outer cover fabric.

A. Gas Valves

Excessive difference of pressure between the interior and exterior of the gasbags has been dealt with in past airships by means of automatic spring loaded valves fixed at the bottom of the gasbags. These valves have to move as the bottom level of the gasbag changes, but when the bag is full they seat on cages which are connected to vertical trunks to carry away the discharged hydrogen to the top of the ship and thence through hoods into the atmosphere. There are obvious objections to these floating valves. These objections quickly increase with size.

There are many difficulties in the mechanical design of a spring loaded valve to deal with high rates of discharge if the valve is to be sufficiently robust without excessive weight and size. It is necessary, for instance, for

[15] *Vide* Giblett, Journal of the Royal Aeronautical Society, Vol. XXXI., No. 198, p. 528.

the valve to open deliberately and reliably at a comparatively small pressure difference and yet it must seat properly with very little leakage at all pressures less than that specified for opening. In order to avoid excessive size the valve should open wide, *i.e.*, the moving part should move a distance equivalent to one quarter of the diameter. To achieve as large a travel as this on a spring loaded valve is a problem of considerable difficulty. Such a valve, operating at a small pressure difference, is extremely sensitive to the local conditions immediately surrounding it, and even if this large travel could be achieved by an unstable linkage the valve would be too slow in closing.

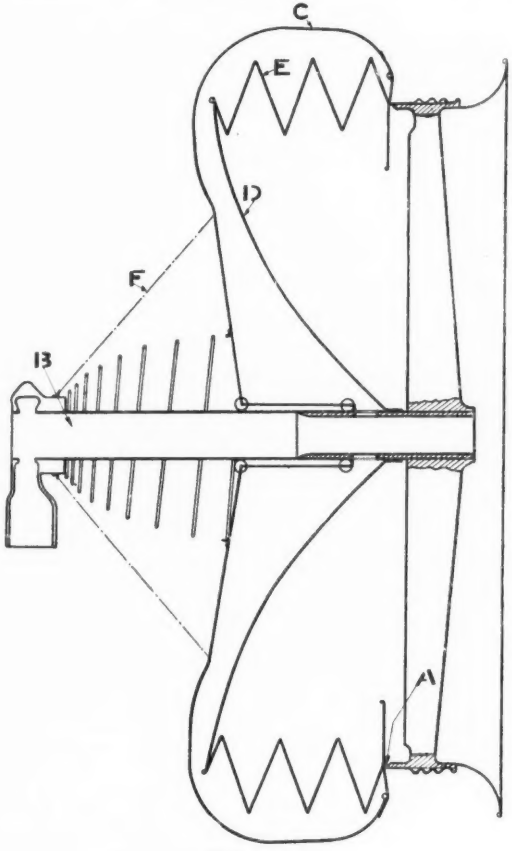


FIG. 17.

Diagrammatic arrangement of gas valve.

It will be obvious that valves designed to deal with a high rate of discharge must be rapid and deliberate in opening and closing, if they are not to discharge more than the required amount of gas.

It was decided in consequence to adopt in R.101 a type of valve with bellows control in the place of springs. The valve and its method of operation are illustrated in Figs. 17, 18 and 19. The position chosen for the valve was

on the side of the gasbag at mid-height, the valve being attached by a petticoat to the gasbag and also to the gasbag wiring. The proportion of disposable lift to gross lift in the airship is such that the pilot will never require to deflate the gasbags to such an exent that the free surface of the gas would be as high as the level of the valves. Thus the disadvantage of using valves which have to move with the free surface of the gasbag is avoided. Another advantage of this position over placing the valves at the bottom of the gasbags is that the pressure difference is greater at this height, and consequently a greater rate of discharge is obtained from a given aperture. Furthermore, since when the valve is not required to open, the pressure is employed to exert a closing force, this level has the advantage of ensuring better seating of the valve.

Two sizes of valve are employed, respectively 30 in. and 40 in. in diameter. In general, each gasbag is fitted with one valve on either side, the smaller diameter being employed in the smaller bags.

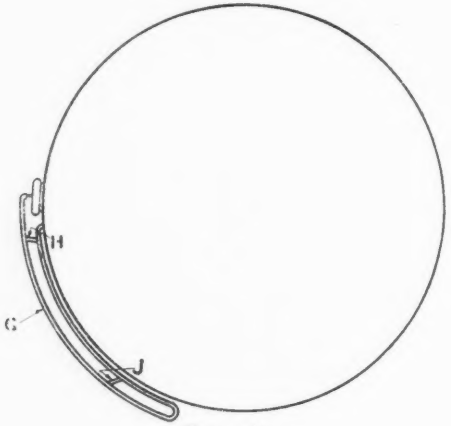


FIG. 18.

Arrangement of gas valve syphon pipe.

A metal seating *A* (Fig. 17) is fixed to a casting in the form of a wheel with four spokes. This is supported by the gasbag wiring. A fabric petticoat on the bag is bound round the rim. From the hub of the casting a tubular spindle *B* projects. Travelling on this spindle is the moving portion of the valve *C*, which is a large spun casing carrying a flexible fabric annulus *D* which forms the seating. Fixed to the spindle is a stationary conical diaphragm *E* within the moving casing and connected to the latter by fabric bellows *F*. Gas has direct access to the inside of the bellows and exerts a closing force on the flexible face, since the effective diameter of the bellows is greater than that of the seating. The valve is opened by the application of a small pressure to the chamber enclosed between the moving casing and the stationary diaphragm, this operating chamber being connected to the gas supply through the tubular spindle *B* and fabric cone *F*.

The operating chamber has a direct leak to the atmosphere which enables the valve to close when there is no flow of gas into the chamber. Only a light closing spring is needed, owing to the fact that the jet of gas discharging exerts a closing force even when the valve is 10 in. away from its seat. No adjustment of this spring is required since small variations in its strength

do not affect the working of the valve. The arrangement of the valve in the ship is shown in Fig. 18. One end of the *U* tube or syphon pipe *G* is connected to the gasbag close to the valve, and the other end is connected to the operating chamber of the valve. The bottom of the *U* tube is approximately level with the bottom of the gasbag, so that when the bag becomes full the gas spills round the tube and up into the operating chamber, thus opening the valve.

Owing to the permanent leak in the operating chamber a certain minimum rate of supply of gas through the "*U*" tube is necessary to open the valve.

FIG 19.
General construction of gas valve.

It was found on test that by proper design of pipes the necessary velocity could be obtained with a pressure difference at the bottom of the bag of only two millimetres of water, at which the valve will open deliberately to its full extension. The rate of discharge of the 40 in. valve under these circumstances is 36,000 cu. ft. per minute.

There are two cross connections in the *U* tube marked *H* and *J*. Connection *H* is a small spring loaded relay valve which can be operated from the control car. When this is open gas will enter the operating chamber of the main valve and open it, no matter what the degree of inflation of the gasbag may be, thus it serves the purpose of the so-called manœuvring valves which were fitted in past airships in addition to the automatic gas valves. The cross connection *J* is a fabric pipe which is normally tied off. If, however, the pilot anticipates a severe buffeting with rapid changes in height above the

airship's ceiling, he can untie this connection, which means that the valves will then operate automatically when the free surface of the gas is level with this connection instead of when it reaches the bottom of the bag. Thus the generation of excessive pressures will be avoided. Since, normally, the airship will become light by the expenditure of fuel as the journey proceeds, it is anticipated that the normal procedure will be for the pilot to undo these connections after the first few hours of flight as a matter of routine.

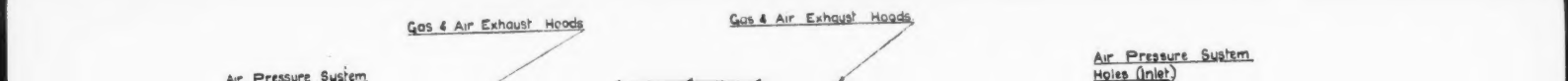
B. Air Vents in Outer Cover

If the airship is rising rapidly and the gasbags are not full of gas, air has to escape from the outer cover. If the airship continues to rise past its ceiling a mixture of gas and air will have to pass through the outer cover, and its rate of egress will be greater because of the lower density of the mixture. So far as rapid ascent is concerned, therefore, the principal problem is to provide sufficient opening for the egress of air at the maximum rate of rise *under all conditions of flight* (*i.e.*, from zero up to maximum forward speed at different angles of pitch).

The problem, however, is more complicated than that of pressure control in the gasbags, since it is necessary to provide for the ingress of air when the ship is descending under all conditions of flight. It was not found possible to devise a type of vent which would be equally suitable both for egress and ingress, and therefore one set of openings is used for the former and an entirely independent set of openings for the latter. It might be supposed at first sight that in order to prevent excessive pressure difference on the outer cover and hence excessive load on the fabric it would be sufficient to merely provide a sufficient number of plain holes of adequate area, and, indeed, some German airships have been fitted with a series of square holes along the underside of the ship covered with netting. It must be realised, however, that when the ship is in forward motion the pressure distribution over the hull is by no means uniform, and the nature of the distribution alters with pitch. Were it not for this, it would be impossible for the ship to gain any dynamic lift. If the outer cover is to be subjected to no pressure difference whatever then the distribution of pressure necessary to give the dynamic lift *will have to come on to the gasbags themselves.*

This would practically mean a flow over the surface of the gasbags equivalent to that which would normally exist over the outer cover. Since this flow would have to pass through the holes in the outer cover there would bound to be a considerable increase in the drag of the airship and also excessive draughts within the hull. Although some internal ventilation is desirable, draughts of this magnitude might obviously prove objectionable for various reasons. Fig. 20 illustrates the nature of the pressure distribution over the surface of R.101 at 10° pitch, and also illustrates the arrangement of openings for dealing with the regulation of the pressure under the various conditions of flight.

For the ingress of air when the ship is descending, a series of inlet holes are provided near the nose and tail of the ship. These are fitted with internal fabric hoods, which act as non-return flaps and also serve to collect and drain any moisture which may enter. It will be seen that these holes are placed in a position where there is always a positive inward pressure if the ship is going ahead, no matter what the angle of pitch. In fact, air will always be entering into these holes when the ship is going ahead, no matter whether it be rising or falling. They provide sufficient inlet for the general ventilation of the hull. They are not quite sufficient, however, to deal with the maximum rate of descent and therefore some additional holes with non-return flaps are arranged at the bottom of the ship at frame 5.


Gas & Air Exhaust Hoods
Gas & Air Exhaust Hoods
Air Pressure System
Air Pressure System
Holes (Inlet)

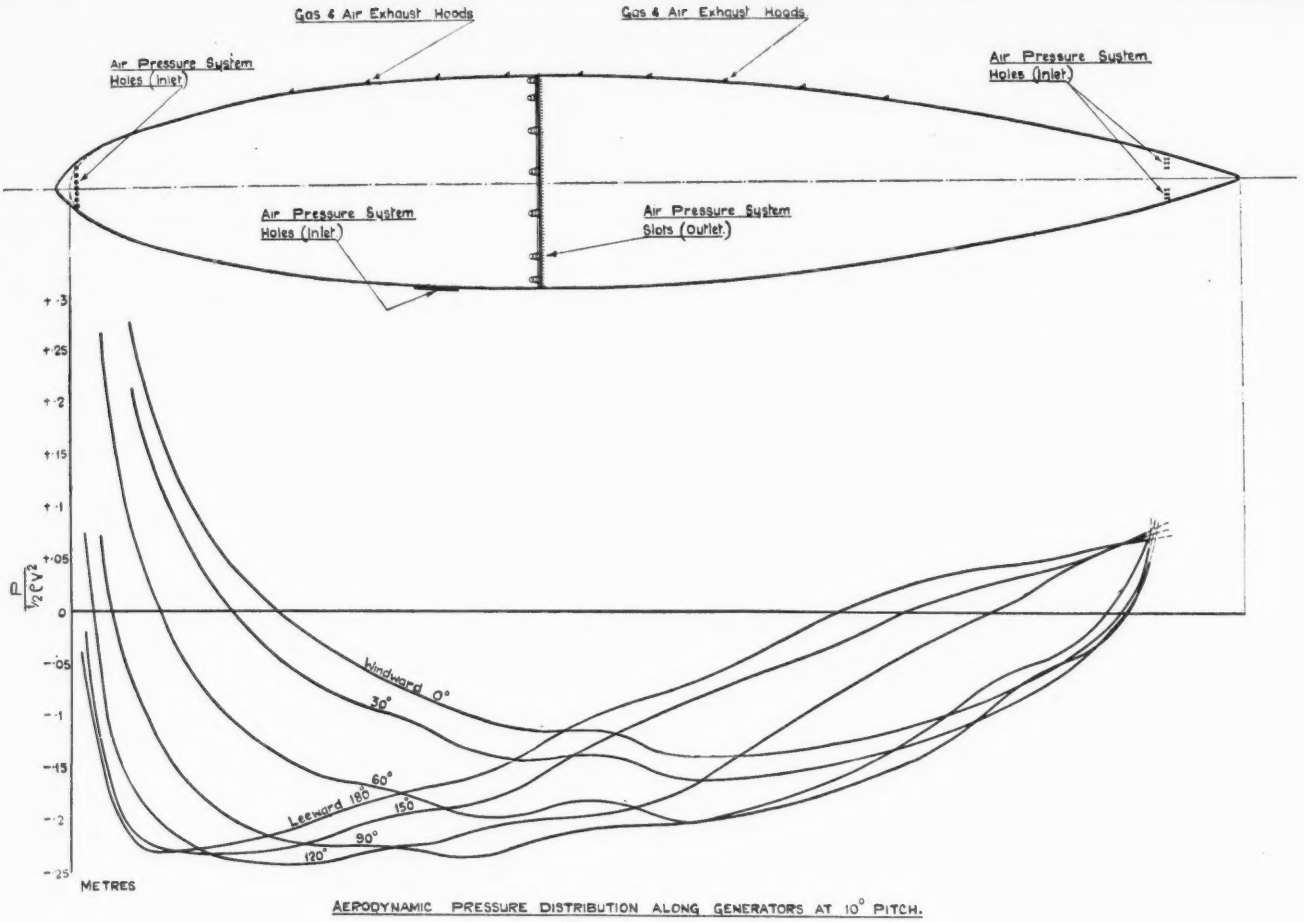


FIG. 20.

Diagram illustrating the distribution of pressure over the surface of the hull and the openings in the outer cover.

For the egress of air when the ship is rising, a series of slots are provided in the cover placed circumferentially approximately at the centre, *i.e.*, round frame 7. This position has been chosen because it is found from a study of the pressure distribution diagrams at different angles of pitch that the *mean* external pressure round the circumference of the ship at this point is approximately constant for different angles of pitch; thus the control of pressure is made more certain by the fact that the air is always escaping at a point of constant mean external pressure. The arrangement of these slots is illustrated in Fig. 21. The forward

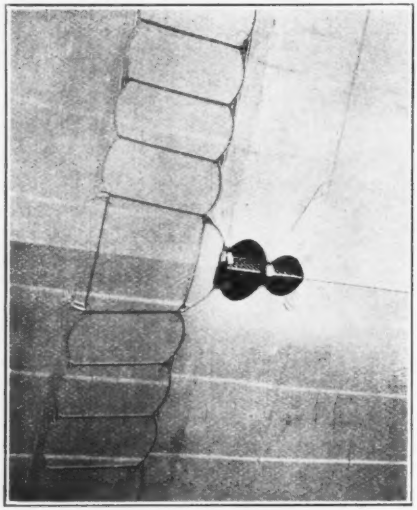
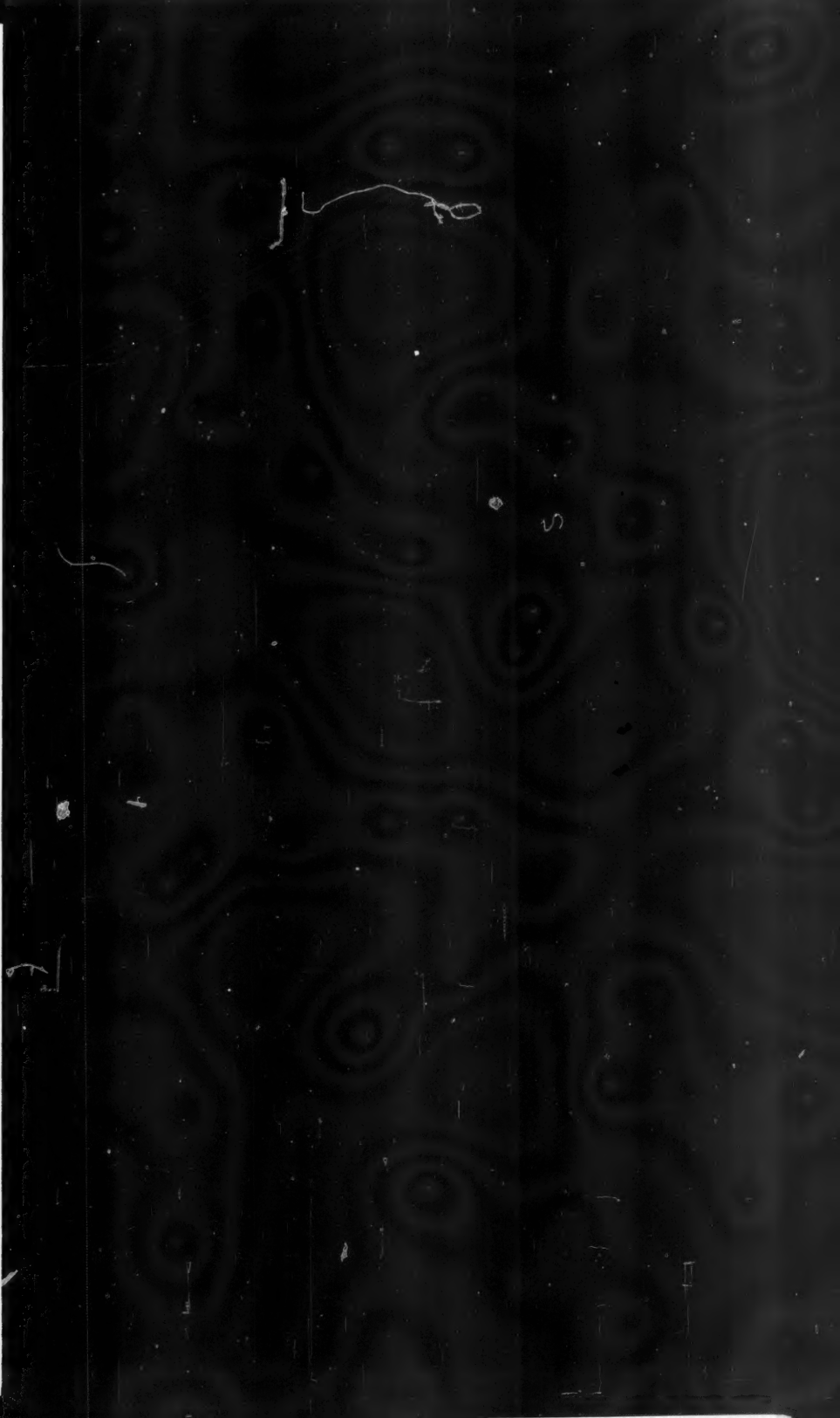
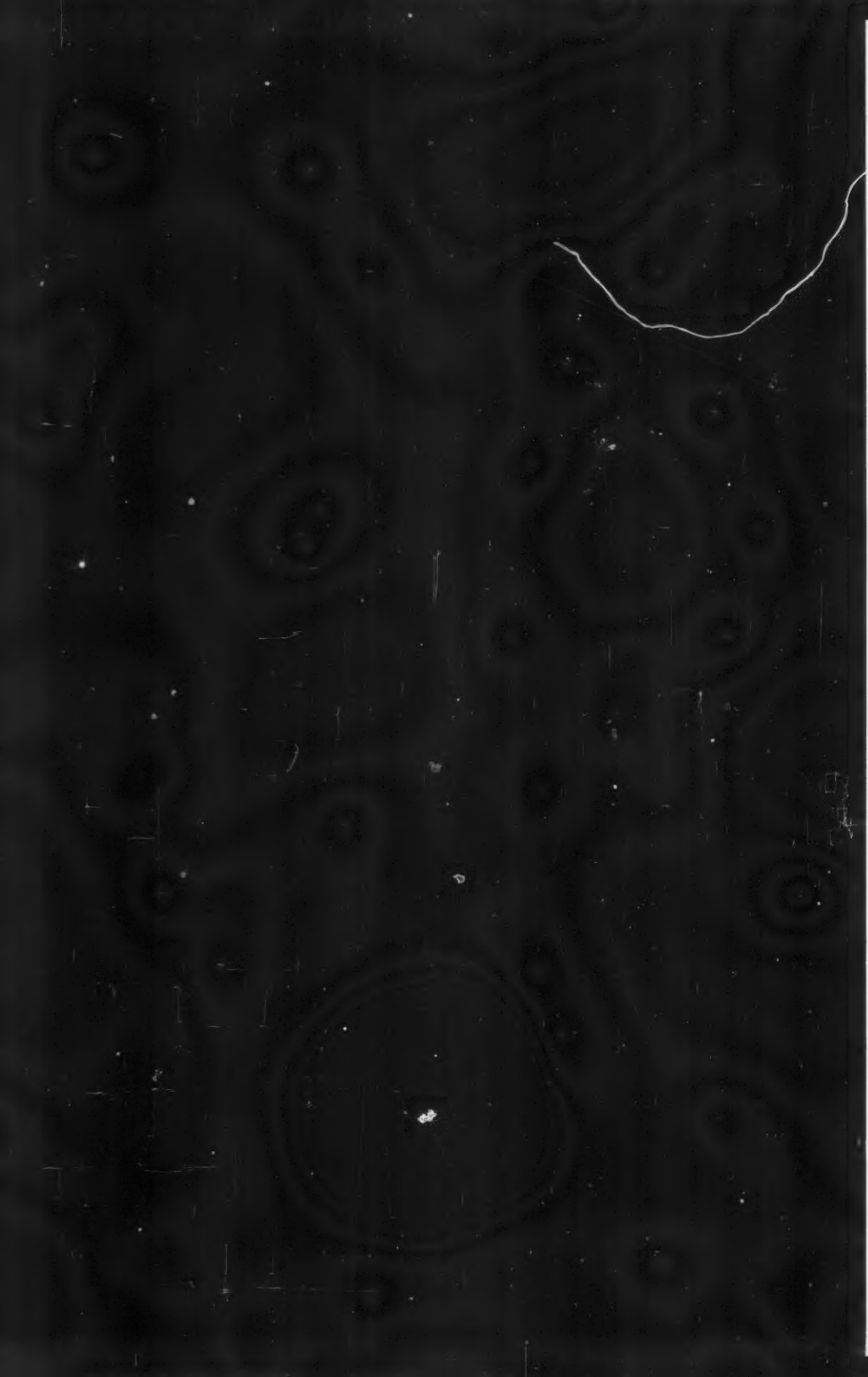


FIG. 21.
Automatic slot in the outer cover.

half of the cover is made to overlap the back half of the cover, the latter being stiffened at its forward edge. The after edge of the forward half of the cover is fitted with springs so that at an excess of internal pressure over external pressure, it can expand away from the back portion of the cover and thus provide an opening through which air can escape somewhat similar to the gills of a fish. This opening can be large enough (without causing excessive pressure difference) to deal with all the air which has to escape from the inside of the hull when the ship is rising, including that which may be entering from the holes in the nose and tail if the ship is going ahead. The slots are, of course, completely closed when the ship is descending. Normally, the main external pressure round this belt of the cover when the airship is going ahead is considerably below zero, *i.e.*, the external air is causing a suction. Were the slots not controlled by springs this suction would cause a low internal pressure which might lead to excessive strain on the fabric. The action of the springs together with the inward feed of air from the holes in the nose and tail avoids this.

In addition to the various openings described above, a series of hoods are fitted along the top of the ship to facilitate the escape of any gas which might otherwise collect there and not be able to get away through the central slots. It is estimated, with the above arrangements, that if the ship is rising at any rate from zero up to 4,000 ft. a minute the internal pressure (not the





local pressure difference which varies from point to point along the hull) will be governed between 0 and 5 lbs. per sq. ft. if the ship is not in forward motion and 0 and 2½ lbs. per sq. ft. at full speed.

The similar figures for the airship falling at any rate up to 2,000 ft. a minute are 0-1½ lbs. per sq. ft. if the airship is not in forward motion, and zero pressure if the airship has full forward speed.

Conclusion

The author is conscious that many things about which information may be desired have had to be omitted owing to limitations of space. It has not been possible to deal with the many interesting stressing and design problems which have arisen. The valuable work of the Stressing Office, Design Office and Laboratory of the Royal Airship Works, and also of consultants, such as Mr. R. V. Southwell, is gratefully acknowledged. Without the ungrudging efforts of all those whose names have been mentioned, and many others, it would not have been possible to achieve the progress which has been described. The author is also indebted to the Air Ministry for permission to read this paper.

The fruits of some of this work can be seen already; the rest must await the launching of the airship in the near future. This event alone can furnish the final answer to much of the criticism which has been rife in recent years. There are certain problems on which further experience can only be gained from actual flight trials, and an extensive and progressive series of such trials has been planned. R.100 and R.101 represent a radical but necessary departure from the airships built during the war, since they have to conform to exacting airworthiness requirements and are intended to carry passengers over long non-stop flights. It is quite unreasonable to expect from the first airship of a novel class a degree of comfort, regularity, and reliability of performance comparable with that which has been established for existing means of transport after many years of experience. It would be quite unjustifiable even, in the opinion of the author, to take the performance of these first ships as an indication of the best that airships can be made to give, as unjustifiable, in fact, as it would have been to take a chance in those directions in which experience is scanty in order to achieve high performance at the outset. Needless to say, the policy in design has been just the reverse of this.

Discussion

Mr. R. V. Southwell: He had long felt the greatest admiration of the way in which Colonel Richmond had set about his task of producing R.101; he thought that the paper they had just heard would explain the reasons for that admiration. Some people seemed to think that a big piece of construction such as this should be entrusted to a committee, but he himself was not a believer in democratic control as applied to engineering design. The right lines were those on which Colonel Richmond had proceeded; he had kept the design centralised in his own hands, but whenever he had found anyone with ideas to offer he had always gone out of his way to listen to them, to weigh their suggestions and to incorporate them if desirable—and he had never omitted to acknowledge any help that he had received.

So far as it was possible to predict success before trials, he would state his belief that on the structural side at all events—*i.e.*, in regard to the hull: he had no inside knowledge of the power plant, every contingency had been foreseen in this design. He could not think of any problem in practical engineering which presented more difficulties than an airship, whether on the aerodynamic

or on the structural side; or of any which called for more ingenuity in the invention of special devices, such as the wire suspension of the gas bags, or these novel and most efficient gas valves—which he had recently had the pleasure of seeing in operation.

One might even say that this design was *too* good, unless more ships were to be made to the same drawings; for the full benefit of the ingenuity which had been displayed in providing for replacement of structural members, and so on, would be derived only when the second and third ship came to be built to the same drawings, using the jigs which had been required for the construction of the first. He asked whether Colonel Richmond could give any estimate of the cost of building a second ship to his designs, relative to the cost of the first; he imagined that it should be very much less.

He asked whether the fine tolerances acquired in the structural members—which reflected great credit upon the workmanship—were really essential to the success of the ship? After all, the question of replacement was involved in the answer; for if it should become necessary to replace a structural member which had become damaged, then although the placing member might be an exact copy of the original members which had become damaged, the hull into which it had to fit would probably have sustained some change of form since it was first constructed.

Mr. Relf: A man who would accept the task of building an entirely new design of airship, involving, as it did, the solution of so many new problems, and could succeed as Lieutenant-Colonel Richmond had done, deserved the very greatest praise. Commenting on aerodynamic considerations, he said the most important were those concerning stability and control. If there were errors in the estimation of drag, no damage to the ship in flight was likely to result, though there might be a loss or a gain of a few miles per hour in the speed of the ship. If, on the other hand, the tail surfaces gave insufficient control or the ship was unstable when expected to be stable, there might be serious difficulties in flight. In connection with previous ships, when one had been able to compare the effect of the fins and controls estimated from model tests with full-scale test results derived from turning trials, there had been remarkably good agreement between the two, in spite of the extremely small scale of the models. Therefore, there was every reason to believe that the stability and control of the new ship would be satisfactory. The drag problem was one of the outstanding difficulties, and one was not very certain of the accuracy of predictions of full-scale drag from model experiments. That problem was being studied very closely, however, and in the near future it was hoped to know more about the phenomena that occur in the boundary layer around such a form as an airship and to be in a better position to predict the full-scale drag. It was fairly safe to say, and it was certainly his personal opinion, gained as the result of his study of the experimental results, that the estimated drag coefficient of the R.101 would not be so far wrong as seriously to affect the performance of the ship.

Mr. Hall (Royal Aircraft Establishment): Lieutenant-Colonel Richmond had not given an adequate idea of some of the difficulties he had had to solve in connection with the design and construction of R.101. As one who had been associated with him in this work, though not in connection with design, he could vouch for the fact that some of those problems had appeared almost insoluble, but Lieutenant-Colonel Richmond had never flinched. In a criticism of the type of joint which had been incorporated in the structure of the ship, he pointed out that an important feature of an airship was that it should attain circularity, whereas all the joints were angular. The difficulty of avoiding angles and of attaining circularity was probably insuperable, but it was a matter which needed very serious consideration as circular work was far cheaper to manufacture. If

one half-closed one's eyes and looked at the structure of the ship in the shed, one would be impressed by the angularity of the girders in the structure. Emphasising the accuracy of the work which had been done by Messrs. Boulton & Paul, he said that when the first frame was placed on the floor of the shed and was adjusted by level, although the diameter was about 130ft., the variation was of the order of only a few millimetres. With regard to the tolerances allowed, he said it was all very well to speak of tolerances of 0.030in. on girders 45ft. long, but there were really no means of measuring accurately a length of 45ft. in an engineering shop. Finally, he asked what success had been attained in the direction of finding a substitute for goldbeaters' skin for the gas bags.

Mr. J. D. North (Director and Chief Engineer, Messrs. Boulton & Paul): With reference to Mr. Hall's remarks, he said that the difficulty with regard to tolerances was largely the inability to decide how much allowance could be made for the elasticity of the members. The tolerances decided upon were adopted in order to ensure that the members of the structure should fit together; they had fitted together well, and possibly some of the tolerances could have been enlarged. The 45ft. girder jigs were set out by end rods and dumpy levels. He did not think that measurement to within an accuracy of 0.030in. in 45ft. was possible. He congraulated Lieutenant-Colonel Richmond upon his paper, and particularly on the fact that the fruits of so many years' work were shortly to be gathered. The task had been a very laborious one, and the contemplation of the ship in the shed must afford great satisfaction to those who had been concerned with its production. Inspection of the actual ship was necessary in order that one might appreciate fully the magnitude of the task and the advances that had been made, and he advised every member of the Society to take advantage of any opportunity they might have to inspect it. The trials which had been so long deferred were at hand, and nothing could be more apposite than the words of Kipling:—

> "When through the gates of stress and strain
> Comes forth the great event."

Finally, he said he was convinced, as the result of his experience in association with Lieutenant-Colonel Richmond, that the manufacture of airships would be definitely a practicable commercial proposition, and there was no doubt whatever that the development of airships owed a tremendous lot to the initiative of Lieutenant-Colonel Richmond and to the tenacity with which he had stuck to his guns throughout many difficult years.

Mr. I. J. Gerard: He had been concerned with the mechanical tests at the Royal Aircraft Establishment on girders and other parts of the structure, and he recalled that there had been no machine in existence which was suitable for making the necessary tests, and that it had been necessary to design a machine for the purpose. From his experience of the tests, he endorsed Major Scott's expression of confidence in the ship, and said that there was very close agreement between the estimated failing loads and the ultimate loads actually carried by the various parts of the structure.

Mr. Bateman (who was on board R.38 during her last flight): He had been in the fortunate position of being able to watch the progress of the development of the airship, though he was not attached to the airship service, and a comparison of the design of R.101 with that of previous ships demonstrated that enormous strides had been made. A number of people seemed to worry a great deal about factors of safety in airships, due chiefly to the failure of R.38. It must be borne in mind, however, that when the R.38 was designed, the aerodynamic forces, which cause an enormous bending moment on the ship, were neglected, but Lieut.-Colonel Richmond and his colleagues had certainly taken those forces into consideration in connection with R.101. At the end of the

enquiry concerning the R.38 it was calculated that during certain ordinary manœuvres the factor of safety of the ship was just over one, due entirely to the fact that the aerodynamic forces had not been embodied in the calculations. It could be assumed, however, that in the R.101 that trouble would not arise. With regard to the gas valve described in the paper, he criticised it on the ground that, although it had worked very satisfactorily on test, it seemed to be very elaborate and to contain a large number of working parts. He would have thought that a spring-loaded, cam-operated valve would have been quite as satisfactory, and that with such a valve there was less chance of trouble arising through careless handling. Discussing the holes to be provided in the outer cover as a means of keeping the pressure inside the ship approximately equal to the external pressure during rapid rises and falls, he said he did not suppose the ship would ever get into an up-current of 40 miles an hour; the Meteorological Office could say definitely where storms would occur, and it would be possible to avoid the stormy regions. There was a large number of holes around the nose of the ship in a very high pressure region, so that there was bound to be a considerable flow of air through those holes, and he believed it would affect the drag of the airship adversely. No doubt there would be quite a breeze through the ship. He asked whether it was the intention to keep the holes open throughout any ordinary trip, or whether it was possible to keep them closed under ordinary conditions and to adopt some quick-release arrangement to open the holes in the event of severe weather being encountered.

Group Captain H. M. Cave-Browne-Cave: In view of the fact that airships had not been flying in England recently, he asked what steps were being taken to ensure that the pilot and crew who would have the honour of flying the R.101 would be able to "get their hands in" and to become familiar with airship flying before undertaking the trials.

Commander P. Harrington Edwards: An important part of the problem of airship construction must be considered as to its possibilities as a commercial proposition, as if it were to become such, investors would hope that a grant might be made by Government, in case they were needed for war purposes; if this were so, the question would arise as to whether it would be practical to arrange for the armament and crew to be in the interior of the airship.

He was also not quite certain whether ventilation of the space for passengers would be satisfactory with the accommodation inside the airship.

Captain Boothby: The new type of gas valve is particularly ingenious, but it would appear to involve certain difficulties in practice. It could be assumed that all valves will, at rare intervals, jam either open or shut, due to the formation of ice, or other causes. In this case any obstruction of the upper part of the syphon pipe would cause the valve to jam shut. Is any means provided for operating the valve by hand under these conditions, or any safety arrangement incorporated, such as a weak patch in the gas bag that would blow out before the bag burst?

In the event of the valve jamming open, he assumed that in between 4 and 5 minutes the gas bag would be half empty, and it might well be that the incident had occurred before anyone knew there was trouble unless some automatic indicator is fitted showing what valves are open. Even then it would take a very active man to get up to the valve and tie it off before the gas had gone.

It would appear, though he hoped he was wrong, that the gas discharges direct into the ring space, where it must form an explosive mixture, before passing into the air, where it forms a lightning conductor. He thought the problem of the safe disposal of the waste hydrogen required early attention, and was not insoluble.

During the war it was the practice for airships to carry parachutes for all on board. One would expect the public to demand that all types of passenger aircraft were similarly equipped in future. This will open up a new problem in the design of passenger accommodation in that provision must be made for evacuating all the passengers in the two minutes that might be available in the case of a serious fire. It would be far easier in an airship with an exterior keel. The exterior keel also allows of a better view. He understood that the new aeroplanes building for Imperial Airways are designed to meet the public demand in this respect, even at the sacrifice of some efficiency, and commercial airship designers will probably have to follow a similar course.

As regards the aerodynamic interference of the power cars, it is interesting to note that in the Gamma, which had swivelling propellers, maximum speed was obtained when the propellers were slightly swivelled down, so taking the slip stream well clear of the envelope. It would be interesting to see if any increase in efficiency in R.101 can be obtained by varying the suspension of the power cars.

Mr. SPANNER (*contributed*): He found little that was new in Lieut.-Colonel Richmond's present paper. His criticisms of the new airships had been set out in "This Airship Business," "Gentlemen Prefer Aeroplanes," and "About Airships," and there was nothing in this paper to cause him to withdraw a single one of these criticisms. Many of them he might well amplify, but there was no need for modification or deletion.

He was not at all impressed by the statement that the structural design of R.101 involved calculations necessitating mathematical work to fifteen significant figures. That he regarded as an admission of weakness in the method of attack on this problem rather than as any matter for congratulation.

He had already expressed grave doubts as to the wisdom of rigidly connecting steel and duralumin in the construction of large and very important main girders. He challenged the point last year, when Lieut.-Colonel Richmond read his paper before the Institution of Naval Architects, and no effective reply was made to his criticism. There was a variation of 60 degrees Fah. in temperatures in this country in one month this year. It is taking the gravest risk, in his opinion, to send this composite girdered monster from this country to India, or to any other place on the other side of the equator. He knew of no other big structure in which there was such an utter reliance upon the ability of the composite girders to absorb the effects of big differences in coefficients of expansion as there was in R.101. This was a fundamental fault in the design.

The description of the passenger accommodation was amusing. This has been repeatedly termed "luxurious" and "comparable to that to be found in ocean liners," by speakers and writers who have been admitted to the confidence of the Air Ministry or who are themselves actively engaged in the Air Ministry. Extracts from Lieut.-Colonel Richmond's paper enable one to get a better perspective.

One reads "Settees, shelves, etc., are arranged round the walls (*of the 60ft. by 32ft. lounge*) so that the passengers are prevented from contact with the fabric portions." Were it not for these precautions, of course (to the necessity for which he drew attention in his books), the passengers would quite probably be shot out through the flimsy fabric bulk-heading when the R.101 behaves as did the "Graf Zeppelin," the R.34, and every other big or small rigid which has ever attempted a long flight.

Lieut.-Colonel Richmond deals loosely with the question of transverse stability. Low fineness ratio is certainly detrimental to transverse stability when it is associated with a general layout such as characterises the R.101. While the distance of the centre of gravity below the centre of buoyancy *may* be greater in R.101 than in a vessel of equal displacement but of greater fineness

ratio and having a passenger car external to the main envelope, he did not believe that to be the case with R.101.

However, even if it were the case, it cannot be denied that the forces acting on the engine cars, the fins, and other external fitments have a greater leverage about the middle line than they would have in a vessel of smaller diameter. Further, and this is a point to which great importance attaches, the passengers have far greater freedom of lateral or transverse movement in R.101 than they could possibly have in the vessel of larger fineness ratio with which he was comparing her.

Lieut.-Colonel Richmond, as head of the Design and Research Station at Cardington, has been responsible for many estimates during the time that he has been concerned with the inception and development of the present rigid airship programme. Those concerned with the time and cost of construction of R.101 have been very far from accurate. He was quite amazed to think that, at the very time when Lieut.-Colonel Richmond said there were only three of R.101's frames erected and three others partly constructed on the floor, he had supplied Sir Samuel Hoare with the information that the R.101 would be flying before the end of 1928, for that was what the Air Minister told the House a year ago. Now Lieut.-Colonel Richmond said that the R.101 is within two months of *absolute completion*.

He had no inside information at all, but after carefully reasoning matters out, he wrote *in 1927* and at a time when the Press were predicting the most marvellous things for R.100, that these ships would not be in the air till 1929.

However, he sincerely hoped that these wonderful "fifteen significant figure" calculations would not prove as faulty as the estimates to which he had referred. For the sake of the probable serious loss of life and also the damage that would be done to British air prestige, he sincerely trusted that both R.100 and R.101 would prove technical successes. He had consistently made specific criticisms in order that his contributions to this vexed question might be as helpful as possible.

Even if they do all that their designers now fondly hope, however, they will remain hopelessly unable to justify themselves commercially. Before the end of 1928 he asked permission to read a paper to the members of this Society setting out reasoned arguments as to why it is the case that the rigid airship, no matter how perfect her design, can never become a commercial proposition. The permission was refused, but he trusted that he would be permitted to put it on permanent record in the proceedings of this Society that the two papers read, the one by Major Scott and the other this present paper, absolutely fail to weaken his case in the very slightest degree.

It will be evident, sooner or later, even to the most ardent airship enthusiast, that the whole of the money spent on these two craft has been wasted. They will teach no lessons that were not set out very clearly, for those who could to read, in the "Log of H.M.A. R.34."

The PRESIDENT: It was really difficult to appreciate the ingenuity they had displayed unless one visited Cardington and studied there the many aspects of the design and construction of the ship. With regard to the tanks used in the ships, he asked for information as to the method of construction, their weight, and the method of slinging them. Referring to the outer cover, he said he had not understood clearly whether it was entirely pre-doped or whether it was to be doped when in place.

REPLY TO DISCUSSION

Commenting first upon Mr. Southwell's reference to the tolerances allowed in the manufacture of the structure members, he (Colonel Richmond) said Mr.

North had hit the nail on the head when he had indicated that the proper way to start was to make the tolerances as fine as they could conveniently be made, and there was no doubt that that policy had paid. The position at the Cardington shed exactly twelve months ago was that three rings only were erected, and we were now within two months of the completion of the ship, so that erection had been speedy. He personally considered, however, that the tolerances allowed had been unnecessarily fine, but he had arrived at that conclusion only as the result of watching the work done, and he believed it would be possible to work to wider tolerances in the future. He hesitated to discuss costs, since so much depended, of course, upon the decision as to which items could be included legitimately when determining the cost of the ship. It was very difficult to determine the cost of experiments which would not have to be repeated if another ship were built, and he quite appreciated the point made that the full advantage of the enormous amount of work which had had to be done would be gained only if and when further ships of the same type were built. Naturally, he very much wanted to build further ships of this type. He agreed with Mr. Relf that *the* aerodynamic problem in airship model work was that of stability and control, and that the problem of drag was not of first-class importance from the point of view of safety. The pressure loading could almost be calculated without recourse to model experiments, but calculations in regard to stability and control must depend upon model experiments. So far as he was aware, the experiments made by the National Physical Laboratory staff had never let us down, and he did not suppose for a moment that their estimates with regard to R.101 were not accurate. Dealing with Mr. Hall's reference to angularity, he said one could not build a huge structure such as the hull of the R.101 without having angles, and the best one could do was to contrive the members so that there was the minimum number of different angles. Mr. North had almost achieved perfection in that respect, but perhaps in time it would be possible to effect further improvements. The work in connection with the production of the substitute for goldbeaters' skin was progressing very well indeed, and he hoped Mr. Hall would soon be able to visit Cardington again in order to see what had been done.

Replying to Mr. Bateman's reference to the gas valve, he said there was only one moving part in it, namely, the one big spinning, as in any other gas valve, and he was afraid that no spring-loaded valve, even if it were one with an unstable linkage, would ever move sufficiently quickly for the purpose in view. If one were to produce a valve to provide an enormous rate of outlet one must ensure very quick action, otherwise one would overshoot the mark to quite a large extent. The number of holes in the cover was excessive, in his view, but he had been instructed to work to the figure of 4,000 cu. ft. per minute, and hoped he would never have to work to it again.

The problem of the training of the personnel who were to fly the ship introduced difficulties. The last flights in this country were those of the R.33, some three or four years ago. Since then, however, a good deal of experience had been gained at the mast by means of a kite balloon which had been let up and hauled in under various conditions, whilst the officers had been undergoing an intensive course of meteorological study and had been "playing the flying game." Each day they studied the meteorological data provided and had decided, in the light of that data, the exact course they would take if flying to any particular place, the exact amount of fuel required, and so on. He felt that that was the best that could be done.

Replying to Captain Edwards, he said the ship was not a military airship; it was not designed as such, and, indeed, he did not know at the moment what functions a military airship would perform. He did not mean to imply that it had no function; indeed, he believed it had, but he would need some direction

as to what were considered to be desirable features before he could say whether or not the ship could be adapted easily for military use. For instance, he would require to know whether it was to carry aeroplanes, whether it was to carry armaments, and, if so, of what kind, and so on. There was nothing inherent which rendered it unadaptable for military purposes.

With regard to ventilation, he pointed out that the ventilation of the passengers' quarters was independently controlled and had nothing to do with the ventilation of the main cover. A special fan was provided to feed air into ducts in the floor, and that air could be heated or not, as desired.

The tanks on the R.101 were of extremely simple design, and were merely cylinders with hemispherical ends, and having no internal structure at all. Each large tank was made to hold about 224 gallons of fuel. There were swages made in the tanks to hold saddles, and the tanks were slung from the saddles to the top member of the ring and were stayed afterwards against pitch and roll. The weight of such a tank complete with its saddles works out at .250lbs. per gallon of contained fuel, and he could not believe that it was possible to make airship tanks of lighter construction. They had been satisfactorily subjected to gruelling tests.

With regard to Captain Boothby's point on the possibility of the jamming open of gas valves, it should be noted from Fig. 17 that the moving parts are entirely enclosed so that the prospect of their being jammed by the formation of ice is extremely remote. It should also be explained that there is a walking way along both sides of the ship at the level of the gas valves so that the crew can quickly reach them in the event of their requiring attention.

Captain Boothby also draws attention to the possibility of the collection of an explosive mixture of hydrogen and air within the hull of the airship. He has apparently overlooked the account given at the end of the paper of the effective means employed to ventilate the air space within the hull. This will sweep out any explosive mixture in a very few seconds.

Mr. Spanner is apparently of the opinion that the present paper does not answer the many criticisms he has set out in his three books on the subject of airships. It could hardly be expected to do so. Many of his criticisms do not touch on the question of the advancement of the science of aeronautics, the promotion of which is the principal object of the existence of the Royal Aeronautical Society and the papers which are read before it.

Other of Mr. Spanner's criticisms were undoubtedly based on a lack of knowledge as to the exact design and construction of R.101 and therefore do not require comment. Yet other criticisms have undoubtedly been fairly answered in the present paper and a previous paper read by the author to the Institute of Naval Architects. Mr. Spanner, however, is apparently not prepared to admit this. The author is content to leave this matter to other judges.

Mr. Spanner repeats his contention that there is danger to be anticipated from temperature effects in a composite structure. He must surely be aware that the hull of the Graf Zeppelin and other airships which have flown through a wide range of temperatures consist of duralumin girders and steel wire bracings. The author showed in the paper given to the Institute of Naval Architects in March, 1928, that for a range of 60°C., *i.e.*, $\pm 30°$ from the manufacturing temperature, the temperature stresses introduced in the composite girders were of the order of 1 to 2 tons per square inch.

Mr. Spanner states that the paper "deals loosely with the question of transverse stability." The paper does not attempt to deal with this question at all, it merely refers to the objection which has been raised to putting the passenger accommodation inside the hull on the score of raising the centre of gravity. Mr. Spanner states "it cannot be denied that the forces acting on the

engine cars, fins, and other external fitments have a greater leverage about the middle line than they would have in a vessel of smaller diameter." In spite of this he apparently advocates putting the passenger accommodation outside the hull, the effect of which would be to increase the forces to which he refers.

The rest of his criticism contains no technical or scientific matter and therefore requires no comment here, except possibly his hope that the fifteen significant figure calculations will not prove faulty. It is made quite clear in the paper that these figures refer to geometry. It is also made clear that these geometrical calculations enabled the erection of the hull, which is now complete, to proceed smoothly and quickly. Further comment would be superfluous.

REVIEWS

Rakcten-Fahrt

By Max Valier. (R. Oldenbourg, Munich and Berlin.)

Although but a small portion of this book is concerned with aeronautics, it is one of the most interesting popular scientific treatises that we have read recently. The main thesis of the book is to examine Mr. Barbicane's project in Jules Verne's "Trip to the Moon" in the light of recent advances in technique, and to consider whether travelling through space is now a feasible proposition. The problem is approached in all seriousness and with German thoroughness. Although the conclusions are favourable as far as is now possible to fire a rocket at the moon—given the necessary financial backing—the whole idea of manned rockets is vitiated by the fact that the initial acceleration would far exceed that which the human system can stand without instant death resulting and, as the author points out, there is no point in adding another meteor to the countless number now traversing space! But what a fine idea to shoot all our waste metal at the moon and be rid of it for good!

Judging by the chapters devoted to the flight of rockets, there is plenty of scope for "model research" in this direction. After describing the history of rocket propulsion, the author concludes with an interesting account of the design (in which he took part) and trials of the rocket-cars, built by von Opel—the millionaire of the famous cycle and motor firm—and of model rocket-planes. The book should be read by all interested in what may become a new motive power in engineering.

Fluid Dynamics for Aircraft Designers

By Max M. Munk. (Ronald Press, New York.)

In spite of its title it is rather difficult to tell from the contents for what class of persons this book is intended. It is scarcely a student's book unless he has a greater working equipment of mathematics than the average; on the other hand, a good deal of the matter would be an old story to the man who had reached the design stage. And yet the book does give in a comparatively short space—compared, that is, to other advanced text-books on aerodynamics—the applied mathematics of the subject, and would be well suited to an engineer who was about to take up the aircraft trade. It is particularly valuable as showing how far mathematical analysis—as distinct from empiricism and dimensional theory—can solve the practical problems of the physical science of flight at the present day.

To this science the author himself has made no mean contributions, and the greater part of the book is devoted to the forces on airship hulls and wings as predicted by the potential flow and circulation theory. The Joukowsky aerofoil, Bénard-Kármán vortex systems, and channel interference are each given allotted short sections, and the book finishes with an account of the principal results of model research.